THURSDAY, NOVEMBER 29, 1894.

LOCOMOTIVE CONSTRUCTION.

The Construction of the Modern Locomotive. By George Hughes. Pp. 260. (London: E. and F. N. Spon, 1894.)

OF all the many branches of engineering, that of locomotive engineering has been generally overlooked by the writers of text-books, and until quite recently the only works on this important subject were the classical works of Z. Colburn and D. K. Clerk, "Locomotive Engineering and the Mechanism of Railways," and "Railway Machinery." These works are more than twenty years old, and do not now represent modern practice, although the rules and formulæ given are largely made use of to this day, besides which the experimental data obtained by D. K. Clerk on the old Edinburgh and Glasgow Railway, some time before the year 1855, may still be regarded as of great value.

Locomotive engineers in this country owe more to the late Mr. William Stroudley than they care to admit. To Mr. Stroudley is due the thoughtful and careful designing of every part of the locomotive, from the valve motion to the damper on the ash-pan, and, thanks to his example, locomotive design has become as near a science as it is possible to be, always bearing in mind that abstract calculations are nearly useless for this purpose. A very important point in the design of a locomotive is that of facility of repairs in the running-shed. It is possible to point to more than one type of British engine where the draughtsman appears to have had entirely his own way in the design, and consequently an ordinary repair, such as changing a spring, entails the partial stripping of the engine in order to lift it high enough to effect this; whereas it should be possible to do it with a couple of jacks, just to ease the weight off the spring, take the pins out, and then replace the spring. Another similar case may be quoted. Some heavy main line passenger tank engines have recently been constructed; and should a copper stay leak badly on the side of the fire-box, the side tanks cannot be removed to get at the leak without cutting out rivets and removing the angle-iron supporting the side platform; whereas had the draughtsman been an engineer, he would have foreseen such an emergency, and provided for it. These examples are sufficient to show that when designing an engine, the position of every pin and bolt should be carefully considered, so that their removal, if necessary, will be an easy matter in the running-shed.

The volume before us, unfortunately, does not deal with design, but treats only with the manufacture of a locomotive in the works of the Lancashire and Yorkshire Railway Company at Horwich. As these works are comparatively new, it is only fair to expect to find the practice thoroughly up to date, and as the author is an assistant in the chief mechanical engineers' department, the information given may be considered to be authentic. The title chosen for the book is a little misleading, because only one type of engine is discussed; moreover, this engine is fitted with the Joy valve gear, a type of motion certainly not generally adopted by locomotive engineers.

The work is divided into six sections, and these are again subdivided when necessary. Each section describes the actual progress of the work done in that section. Taken as a whole, this book is unique; it is the only one we know of that appeals to the locomotime engineer in the language and phraseology of the works, and without the cant usually found in text-books of to-day dealing with mechanical subjects. The book being of a thoroughly practical nature, it may be as well to follow its contents in order.

Section i. deals with the boiler, the materials used in its construction, and the methods of manufacure. Steel is the material used for the shell in most cases. The author observes that if a plate is buckled, this buckling is got out of the plate by planishing, or by a multiple roller straightening machine. The former method cannot be recommended for boiler-plates, but for tank side, cab, or splasher-plates it is the common practice. The question of locomotive boiler drilling is thoroughly well gone into, but the methods described are not those of most recent practice, which may be concisely stated to be—the edges of all plates must be machined, rivet and other holes to be bored after rolling the plates to shape, and drilling all holes out of the solid. Many complicated multiple drilling machines have been constructed to meet these requirements, but it is questionable whether such complicated tools are necessary, especially when the self-pitching attachment is used, because the holes in each seam must be set out in order to find the position of the holes for the "holding together" bolts, which are usually placed 12 to 14 inches pitch, and which are usually drilled through a template in bunches before the plates are rolled. Further on the author describes a plate-bending machine capable of rolling a plate to the very edge. As such a machine is certainly very badly wanted it may be here questioned whether the one described is really capable of doing this. The old method of setting the last 4 or 5 inches with a "former" has long been condemned, but with vertical rolls the ends of the plates may be rolled to the true radius by means of a curved packing piece which holds the plate up to the movable roll, resting against the smaller live rolls behind.

We are told that the inside and outside butt strips are sheared to size, and not machined on edge; unless they are afterwards annealed, this practice cannot be commended. One cannot help being astonished at the free use of cast-steel in the boilers. This is very much up to date; but what gain there can be in using a cast-steel tee-bar in place of a rolled one for the longitudinal and sling stay attachments, cannot be seen. The crown of the fire-box is stayed with cast-steel girder-stays or roof-bars, and ferrules are used to preserve the proper distances. Surely these ferrules might be replaced by bosses cast on the stay-bar, into which the bolts would be screwed, thus strengthening the bar, and being less complicated. The foundation rings are also of cast-steel; these castings evidently require much setting and "paening." Surely they are machined inside and out, as required by good practice?

The paragraphs dealing with flanging plates and the tools used, leave nothing to be desired; they are concise and to the point. Reasons might have been given for the adoption of the "Webb" fire-hole; it seems to

entail very severe treatment of the plates for no obvious purpose. There are many ways of placing the copper stays in position, and the author evidently cuts off the ends after screwing them home. Here he is behind the times: "modern practice" requires them to be cut to length in the lathe, afterwards being screwed home by a sort of stud-driver. This does away with any chance of injuring the threads by cutting them in position by eccentric portable shears, or the more barbaric hammer and chisel.

The quadruple tapping machine (illustrated) no doubt does its work very well, but it must be moved from boiler to boiler, and this entails much labour and loss of time; a far simpler tool is generally used, namely, a light radial jib carrying a movable carriage, over which the ropes run to the tool-holders, all the compensating gear being on the wall. Taken as a whole, Section i. is extremely well written, and covers a large field of detail; it concludes with the testing of the completed boiler. The hydraulic test of 200 pounds per square inch is much below the usual test-pressure in water, viz. one and a half times the working pressure, for new boilers.

Section ii., occupying eighty-seven pages, is divided into three parts. The first part deals with the iron foundry, the second on the use of steel castings, and the third describes the brass foundry. It is evident, after careful perusal, that much attention has been given to this particular branch of the works, especially the careful manner in which the mixings of the different metals is carried out, and the valuable illustrative tests showing the necessity for annealing steel castings. As an example of foundry practice, we find a full description of the moulding of a pair of twin cylinders, probably the most difficult and important piece of work occurring in a railway foundry; a few other examples are also given. In part two of this section the author wisely suggests that engineers should formulate a standard specification for the grade of material for steel castings for locomotive work, and recognise a standard size for both tensile and bending test bars. With this we cordially agree; specified tests vary far too much, and in some cases tend to make the specifying engineer a laughing-stock for contractors.

Section iii., dealing with forgings, is divided into two parts. The manufacture of tyres, axles, coupling rods, and smaller details, is well described, the usual tests being given. The method of work is illustrative of modern practice, particularly the stamping of detail work under the drop hammer. Engine tyres for India usually are required to have a minimum tenacity of from 42 to 44 tons, and with a sectional area of test piece of ½ square inch. The extension, measured over a length of 3 inches, must not be less than 20 per cent. The second part of this section deals with the manufacture of springs, among other things. Page 164 illustrates the means taken to weld a lever on to a shaft; figures C and F may make a "job," but it is preferable to fit the lever end half through the shaft before welding—that is, for a brake or a reversing shaft; for a damper shaft, a dab weld may be good enough.

In Section iv. we find a description of general copper-smith's work; and in passing we may observe that the hemispherical tops for steam dome casings are in some works made from mild steel plates in dies under the hydraulic press, as well as the upper parts of safety-valve casings and corner mouldings for fire-boxes. This reduces the cost of these items considerably.

The machine department of a locomotive works is always interesting. The machinery is in many cases of a special nature; and in railway works, where duplication is said to exist, the machines may be still more of a special type, because only one class of locomotive is made. The Horwich Works appear to be largely fitted with milling machinery, to judge by the amount of care the author takes in his descriptions. Whether milling in its competition with planing, slotting and shaping machines, will ultimately prove the cheaper process, remains to be seen. On page 204 we find the statement that all frame-plates are put on the levelling table and straightened (levelled ?) by the aid of two hydraulic jacks; further on we read that the frame template has been given up, owing to its liability to become distorted, and that a man can draw in a frame in two hours: again, it is stated that a batch of eight frames is slotted, firstly by roughing out, and secondly by a finishing cut. Surely this cannot be called modern practice? To thoroughly and truly level a frame-plate it is necessary to heat it to a cherry-red heat, and level it on a plain surface; for this reason it is usual to punch the frame-plate roughly to shape before being levelled. This punching to shape allows the frame-slotting machine to commence at once roughing out the bunch of plates. Moreover, the plate is more or less annealed by the furnacing.

The frame template generally used is made in three pieces dovetailed together, and the angle-iron bracing is conspicuous by its absence: very little trouble is caused by such a template. Surely the author has made some mistake in stating that a man can "mark out one frame, which is a two hours' job." To mark out a frame from a drawing, without a template for slotting and drilling, will take one man two days or more. Moreover, the frame-plates are not said to be planed on one side. If this is the case, much time is lost in erection and fitting the horn-blocks, assuming of course this fitting is properly done. Such a frame-plate can be planed in from three to four hours.

Case-hardened wrought-iron axle-boxes are said to be things of the past! The Midland Railway Company use nothing else, and many new engines for other railways are being fitted with them; in fact, such boxes working in chilled cast-iron or cast-steel guides are hard to beat.

The last section of this interesting book deals with the final erection of the engine, and we are glad to see that Horwich does not go in for throwing an engine together in ten hours or more, to be afterwards re-erected when the "wonder" has ceased to be talked about.

It appears that the practice of fitting the horn-blocks to the frames when in a vertical position is followed. This cannot be commended; it is far easier for the men to do it before they reach the erecting-shop, when laying on trestles; the holes can then be opened out, and the bolts or cold rivets driven in comfortably. Pianoforte wire has long been abandoned for squaring over cylinder centre-lines by contractors, for the reason that it can be bent between the fingers, and

thrown one way or another by the men. The three-inch tube for setting the slide-bars cannot be trusted for accurate work, owing mainly to the end drooping by its own weight, and slackness in the glands and temporary front cylinder cover. Squaring over the frames is now done by a long square, one arm being placed through the driving horns, and held against them. The distance is then measured from the leading and trailing horns to the other arm of the square when held in the four positions. If the frames are square, these measurements should agree, if the horns are similar throughout. A woven silk cord is best for lining up the frames and cylinders.

The volume concludes with a lengthy description of the Joy valve gear, a gear not generally used in locomotive work. It is a pity the author does not treat the so-called Stephenson link motion in a similar exhaustive manner. In conclusion, we must congratulate the author on having written the first readable and accurate book on the construction of the locomotive engine. He has treated his subject in a masterly way; he describes, as a rule, the most recent practice in a thoroughly professional manner. The work is well and copiously illustrated, and will be of great use to those who take an interest, either professionally or in an amateur way, in the construction of the locomotive. N. J. LOCKYER.

INDO-MALAYAN SPIDERS.

Malaysian Spiders. By Thos. and M. E. Workman. Parts 1, 2, and 3. (Belfast: Published by the Authors, 1894.)

THE material upon which this work is based was obtained by the authors during a recent visit to the East Indies; and since the entire collection has been submitted for examination and description to Dr. Thorell, who has made a special study of the Arachnida of this quarter of the globe, it may be taken for granted that the species have been as satisfactorily identified as is possible. Most of them, whether old or new, have been already described in detail by this specialist. But his systematic zoological work, although in its way of unrivalled excellence, is open to two objections. We have, in fact, heard it alleged, firstly, that the span of human life is too short, and the number of existing spiders too great, to admit of deserving attention being paid to his exhaustive descriptions; and, secondly, that a deal of vexatious trouble and valuable time might be saved by the addition of a few figures to the overwhelming amount of text. It is evident that Mr. and Mrs. Workman have realised the full force of these two objections; for this book of theirs may be briefly described as a supplement designed to make good the defects in Dr. Thorell's report upon their collection.

The work is being issued at intervals in shilling parts, of which three, comprising in all twenty-nine plates, have up to the present time appeared. Every species is illustrated by a hand-coloured figure, together with outline sketches of structural details; and accompanying each set of figures is an explanatory page of text, giving the name and synonymy, the affinities and distribution of the species, and some measurements of the type-specimen. Moreover, in some instances interesting items of news respecting habits, &c., are added; and in the case of the orb-weavers, a figure of the web characteristic of the species is engraved on a separate plate. It is this part of the work, we feel sure, that will prove of the greatest interest to the student of spider-life. Even the pure systematist may learn from it valuable facts bearing upon his aspect of the subject. For the figures and descriptions of the webs afford indisputable evidence that remotely allied genera may construct snares of substantially the same kind, as may be seen by a comparison of the nests of *Callinethis*, *Gea*, *Argyroëpeira*, *Epeira*, and *Gastracantha*, represented in parts 1 and 3; and that within the limits of the same genus, species may be found that spin webs differing widely in important points of structure, as a glance at the figures of the webs of *Epeira calyptrata*, *unicolor*, and *beccarii*, in parts 1 and 3, will show. Clearly the importance of these facts must be steadily kept in view by those who base their classification of spiders on the structure of the webs.

The discovery of the snare of *Uloborus quadri-tuberculatus* has given rise to a curious problem. This web is always spun on the pine-apple, and is of a peculiar basket-shape, the peculiarity consisting in the remarkable adjustment that is exhibited between the structure of the web and that of the plant. But the pine-apple is a native of South America, and has only of late years been introduced into Singapore; so that if the spider is truly a native of the latter place, it has evidently rapidly modified its spinning instincts in response to the slight change in its environment brought about by the introduction of the pine-apple. Before such a conclusion, however, can be looked upon as an established fact, evidence must be produced that the spider and the plant were not concomitantly brought from the Neotropical to the Oriental region.

Another very interesting fact is noticed in connection with *Argyroëpeira striata*. We are told that this spider, which is normally of a bright golden tint, "has the power of darkening down its brilliant colouring when frightened." (part 3, p. 19.) The importance of this observation is greatly enhanced by the independent discovery made by Mr. H. H. J. Bell, and published in NATURE (vol. xlvii. p. 558), to the effect that a West African species of *Argiope* possesses the same faculty of rapidly varying its colour under the stimulus of changing surroundings.

From what has now been said, it may be judged that the value of this book, as an addition to the literature of spiders, is both great and unquestionable. But it is impossible to shut one's eyes to the fact that its general excellence is slightly marred by a few blemishes, which, at the risk of appearing ungrateful, we think it our duty to point out; not, be it understood, with the object of fault-finding, but in the hope that none of them may be copied by other authors, and that some, at least, may not reappear in succeeding parts of the work.

In the first place, respecting the method of publication, it is a pity that both preface and introduction have been altogether omitted, and that species belonging to such widely different families as *Oxyopidæ*, *Attidæ*, *Thomisidæ*, and *Epeiridæ*, should be indiscriminately mixed, as they have been in part 1. With regard to the preface, we hope that the authors will see the application of the maxim, "Better late than never"; and although the adage, "What is done cannot be undone,"

holds good in the case of the parts already issued, we should like, nevertheless, to see a little more method displayed in the grouping of the species contained in the forthcoming parts.

In the second place, since more than one species is now for the first time made public property, it may prove as serious an oversight as it is an irreparable one, that the exact date of the issue of the several parts has not been permanently recorded on the title-pages. Moreover, these new species, instead of being rendered conspicuous as such by the familiar symbol *n. sp.*, are ascribed to Dr. Thorell, with the simple addition of the words "Thorell MS." No doubt the considerations of courtesy expressed by this ascription are worthy of all praise; but it will be as well to bear in mind that the species are for the first time described and figured in a work, not by Dr. Thorell, but by Mr. and Mrs. Workman. It is, consequently, within the bounds of probability that some of us may feel inclined to question the right of the latter two authors thus to constitute the former the founder of these species; seeing that his sole claim to the title rests upon an unpublished suggestion respecting their names, coupled with a privately expressed opinion that they were new forms.

We should also like to suggest that a little more precision in the printing of the figures would greatly add to the value of the plates, without much increasing the cost of their publication. If this and the other alterations we have ventured to propose are adopted for the remaining parts, it is certain that the work, when complete, will rank as one of the most important contributions to the natural history of spiders that has appeared in the last quarter of this century. R. I. POCOCK.

THE PLATEAU REGION OF SOUTHERN FRANCE.

The Deserts of Southern France; an Introduction to the Limestone and Chalk Plateaux of Ancient Aquitaine. By S. Baring-Gould, M.A. With illustrations. In two volumes. (London: Methuen and Co., 1894.)

THE region described by Mr. Baring-Gould, lies, roughly speaking, to the south and south-west of Auvergne, forming a kind of border-land to that country and the Cevennes. Most of it goes by the name of Les Causses. This is a limestone region, furrowed deep by gorges, and pierced by caves. On the eastern side it rises, in three steps, from the neighbourhood of the Gulf of Lyons to the central *massif* of ancient granite and schists and of comparatively modern volcanic-rock; on the western side it falls, in like manner, to the sandy lowlands on either bank of the Garonne.

The scenery of these gorges is always striking and often grand; the finest cañons being those traversed by the Lot and the Tarn. Mr. Baring-Gould's first mention of the former river may serve as an example of the characteristic scenery.

"Near its cradle it passes under the frowning Causses of Sauveterre, then it cleaves the limestone of the Rouergne, and afterwards winds and writhes like a serpent through the Causses of Quercy. Everywhere, at every stage, it affords surprises; the scenery is sublime and quaint. On both sides the cliffs are encrusted with castles and domestic habitations, built half into the crags. Churches and towns stand on the tops of the cliffs, and look down on the boats that glance by. In its sinuosities it washes overhanging scars, without leaving soil at their feet on which to plant a foot, whereas an alluvial meadow, rich and rank, is on the farther bank; then, suddenly, the capricious river turns to the opposite side and treats it as the first. Consequently a road was only to be carried up the Lot valley by means of tunnels and bridges."

Among the natural wonders of this region, its caves and swallow-holes are not the least. These Mr. Baring-Gould seldom ventured to examine in person, but he quotes extensively from M. E. A. Martel, one of their most adventurous explorers, and gives some excellent illustrations. The latter remind us of similar features in the Carinthian Alps to the north of Trieste, and in the Carboniferous Limestone districts of our own country, with which Prof. Boyd Dawkins has made us familiar in his book on "Cave Hunting." In exploring these underground regions a human interest is not always wanting; for the investigator may come across the skeleton of a suicide or the remains of prehistoric man, while the stalactites are sometimes remarkably fine.

It is unfortunate that Mr. Baring-Gould did not get some friend, more expert in geology, to look over the proofs of his book. His references to that subject are often wanting in clearness and precision, and thus are sometimes rather perplexing. This is perceptible even on the title-page, where he speaks of the "limestone and chalk plateaux." But "chalk" *is* a "limestone," so we conclude that the author uses the term in a limited sense, whether it be the descriptive or the geological. But if the former, then, so far as we are aware, the soft white limestone which we call chalk does not occur in the Cretaceous system of Southern France; and if the latter, some distinctive epithet, such as "Jurassic," should have been inserted before "limestone." Such a statement as this also is puzzling: "The lowest stage (of the Causses) . . . is of chalk with a layer of lias above it in places." This is incomprehensible, unless Mr. Baring-Gould uses chalk merely as a synonym for light-coloured limestone (which he often does, unless we misunderstand him), or some strange faulting has occurred (which seems highly improbable). The following passage, also, will hardly satisfy either a zoologist or a geologist:

"The Dolomitic limestone is held to be coral rock built up under water by the industrious insect that is at present forming reefs and islands in the Pacific. At the time when these tremendous masses were composed, the lias lay at the bottom of a warm shallow sea, and on its banks the coral worm worked. Gradually the bottom of the sea sank, and as it sank, so did the insects build upwards towards the light and warmth. After a lapse of ages the whole was upheaved. . . . As the construction is vertical, the structure is vertical, and as the coral insects twisted and turned about sponges, masses of seaweed, and avoided cold currents, the whole mass of rock abounds in hollows in which water accumulates, and in passages through which rivers run."

These, however, are trifling blemishes, which can be readily put right in a second edition. The book is delightful reading, and is full of interesting information, at which we have only time to glance. Mr. Baring-Gould, among other things, gives a good account of the curious fire-hills of Cransac, produced by spontaneous com-

bustion, in the coal basin of Decazeville and Aubin. His description also of the irreparable mischief wrought by the reckless destruction of forests, is well worth reading, for it must be remembered that the weird desolation of the limestone plateaux is a thing of comparatively recent date, and an indirect consequence of the French Revolution. Of the rock shelters of the "reindeer age" in the valley of the Dordogne and of other rivers, he has much to say, and of the dwellers in "holes of the rock" down to the present day; for these caves have been enlarged, or faced with masonry, or actually excavated, at various dates, and in some cases are still inhabited. Of the dolmens and other megalithic remains which are common on the plateaux region, Mr. Baring-Gould writes as one who has made a study of the subject. Perhaps some of his ethnological speculations may not meet with universal acceptance, but they are, at any rate, worth considering. The book contains many curious bits of folk-lore, as we might expect, and narrates sundry remarkable historical episodes in the mediæval struggle between France and England, and in the sanguinary conflicts of Huguenots and Romanists. A chapter is also devoted to the romantic, though often discreditable, story of Joachim Murat, who was born at a dirty little "bastide" of the same name on the Causse de Gramat, near the source of a tributary of the Lot. The book, in short, while it indicates the author's cultivated tastes and wide range of reading, directs the attention of travellers to a region of singular and varied interest, which hitherto has received but little notice even from the French themselves. It is only inadequately described in Reclus' great work, "Géographie Universelle." It has not, however, escaped the indefatigable emissaries of Baedekker, who gives, in the volume on Southern France, a succinct account of the district, evidently founded on personal knowledge. Armed with the little red book, and Mr. Baring-Gould's more bulky volumes, a rich reward undoubtedly awaits the visitor. The guide-book will direct his steps aright; Mr. Baring-Gould's pleasantly written and admirably illustrated volumes will give him abundant information about the chief points of interest, whether physical, archæological, or historical, and will be an unfailing resource during those hours of enforced leisure, which, on a journey, are apt to become tedious.

T. G. BONNEY.

OUR BOOK SHELF.

An Elementary Treatise on Theoretical Mechanics. Part I. Kinematics. Part II. Statics. By Alexander Ziwet, Assistant Professor of Mathematics in the University of Michigan. (London and New York: Macmillan and Co., 1893.)

AMERICAN mathematicians have always followed the system of the French and continental school, so that the progress of the American student in analytical development has not been arrested and stunted by the excessive reverence of the Newtonian methods prevalent in this country.

According to the continental system a student is introduced at the earliest possible stage to the Cartesian methods of geometry and to Leibnitz's extensions in the domain of the Differential and Integral Calculus; and then, even with a comparatively small equipment of analytical knowledge, hardly extending beyond an acquaintance with the notation, he is prepared to study and appreciate a work like the present; while the English student is kept back by clumsy antiquated methods, on the pretext of developing his geometrical and general reasoning powers.

This work is intended as an introduction to the science of theoretical mechanics, adapted to the particular wants of engineering students who, with the characteristic practical energy of their race and age, will not desire to be kept marking time over the rudiments.

The general treatment of the subject is elegant and complete, and valuable collections of illustrative examples are introduced at the different stages. One of these, however (ex. 6, § 276), caught the eye, as requiring amendment; as also the Fig. 29 of the catenary.

An old friend, the problem of the beam in a bowl—in other words, of a spoon in a teacup—given as ex. 20, § 151, deserves separate discussion, and a complete solution in the text.

The present opportunity is favourable for expressing to Prof. Ziwet the thanks of mathematicians in this country for his valuable Report of Prof. Klein's Lectures on Mathematics, called the "Evanston Colloquium," held before members of the Congress of Mathematics in connection with the World's Fair at Chicago, at North western University, Evanston, Ill.

G.

By Order of the Sun to Chile to see his Total Eclipse, April 16, 1893. By J. J. Aubertin. Pp. 152. (London: Kegan Paul, Trench, Trübner, and Co., 1894.)

TWO years ago Mr. J. J. Aubertin, having seen a copy of NATURE for October 13, 1892, containing a letter on the then coming solar eclipse, went home and dreamed a dream. In his vision the Sun visited him and ordered him to gird up his loins, and go to the desert of Atacama and watch the eclipse. This brief explanation is necessary in order to account for the rather clumsy title of the book before us. Mr. Aubertin, regardless of the belief that dreams should be reversed, and that he was seventy-five years of age, travelled to Chile, and, meeting Prof. Schaeberle there, became one of the eclipse party. He was, however, more an interested layman than a scientific observer, and therefore his book is of very little value to astronomers. In fact, the book is chiefly taken up with tittle-tattle of interest to very few beyond the parties concerned. A picture of the corona, as seen by the author, is very pretty, and compares favourably with the impressions recorded by observers of the phenomena before photography monopolised the field as a coronal artist. But at the present time, the results of visual observations of the corona are regarded with suspicion, and rightly, for they never afford any very definite information as to the true form and structure of the sun's surroundings. However, Mr. Aubertin faithfully records what he saw, so his observation must be accepted. The book contains Prof. Schaeberle's photograph as a frontispiece.

Reise nach Südindien. Von Emil Schmidt. Mit 39 Abbildungen im Text. (Leipzig: Wilhelm Engelmann, 1894.)

HERR SCHMIDT'S book is a plain, straightforward narrative of a tour through Southern India, in the course of which he visited Madras, Travancore, made an excursion to Cape Comorin, proceeded by Trivandrum to Cochin, and thence by Coimbatore to the Anamalay Hills, going afterwards to the Nilgiris, and finishing at Calicut. The object of the journey was mainly to study the native peoples, and numerous ethnological photographs give a certain value to the book. There is, however, nothing new in the way of an important contribution to science in the work, which is most interesting as showing the impressions produced on an intelligent and observant German by a visit to Southern India. The style is lively, but perfectly serious, and cannot fail to be of much value in Germany, where it appears few books have been pub-

lished dealing with the lighter aspects of Indian life. It is pleasant to note that Herr Schmidt found the English officials and planters everywhere very hospitable and cordial, ready to assist him in his inquiries as to the people, and able to give him much valuable information on the subjects which he was studying.

By Vocal Woods and Waters. By Edward Step. Pp. 254. (London: Bliss, Sands, and Foster, 1894.)

A RESURRECTION book, made up of papers originally contributed to *Good Words*, *Leisure Hour*, *Sunday Magazine*, *Silver Link*, and other periodicals. The author is well known as a close observer of nature, and he has the amount of poetry in his composition essential in a writer on popular natural history. The book is nicely printed and illustrated.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Acquired Characters.

IN reference to the question as to how far the signification of the term "acquired characters" may usefully be extended beyond the precise limit given in Lamarck's two laws (quoted in my letter of November 15), or what phenomena may be brought into close relationship with those indicated by Lamarck, the following considerations, will, I think, be found useful.

Let us consider a relatively stable local "race" of some species of organism. The race is found to present a certain range of variation, but has an average character; and cases approximating to the average are so far more numerous than departures from it, that for our immediate purpose we may leave the aberrant individuals out of account. The average specific character is a matter which may be determined by measurement and weighing. It can be stated in numerical form as to length of such and such parts, breadth and depth of other parts, weight (*i.e.* amount) of pigment or other chemical product here or there. We know by experiment that these quantities can be altered in immature individuals *within limits* by changing the physical conditions in which the individual is placed. These physical conditions are (roughly speaking) such measurable quantities as those relating to temperature, light, mechanical strains, moisture, and varying amounts of chemical compounds or elements operating on the organism through its absorbing surfaces (respiratory organs, digestive organs, integument).

From such experiments we are led to conclude that without destroying the life of an individual many characters can be increased in quantity, but that there is a limit beyond which they cannot be so increased; and that many characters can be reduced in quantity in the same way in an individual without destroying its life, but that here also there is a limit. Hence it seems that we are justified in distinguishing the "potential" from the "actual" characters of an organism.

This *potentiality* of the individual is something inborn or congenital. On the other hand, the *actual* quantitative condition of the average characters of a naturally-occurring assemblage of individuals is necessarily to some extent the result of the operation of the measurable physical agencies which constitute the normal environment of the species or race. (The range of difference, it may here be noted, between the potential and the actual characters of a species as thus indicated, is found to differ very greatly in different species and in regard to different parts of the organism.) Thus then every individual exhibits certain quantitative characters, the amounts of which are *determined* by the operation on the individual of given and related quantities of external agencies. To a character thus quantitatively determined, some writers have extended the term "acquired character," inasmuch as it is not the congenital potential character in its purity (if such a thing were possible) which we thus contemplate, but the congenital character as moulded, increased, or restrained by surrounding conditions. But whilst I am very decidedly of opinion that a consideration of this moulding, expanding, or restraining influence of the normal environment is likely to throw important light upon the implications of Lamarck's doctrine, I agree most emphatically with Mr. Francis Galton in thinking that the use of the term "acquired characters" must be limited, as indicated by Lamarck's own statement, to characters, "which are regularly found in those individuals only which have been subjected to certain special and abnormal conditions" (to quote Mr. Galton's words). The word "acquired" was used by Lamarck, and should continue to be used as pointing to an acquisition, under *new* conditions, of *new* character or characters, distinct from the normal characters which form, as it were, the starting-point, however determined or brought into existence.

It is, however, true that the difference between the actual characters of an individual organism as compared with its potential characters—a difference the origin of which may be expressed by calling the former "responsive characters"—is of the same order whether the actual characters are determined in their amount by the normal environment of the race, or by abnormal quantitative changes of that environment.

And it seems to me, that in considering this we are led to the conclusion that the second law of Lamarck is a contradiction of the first. Normal conditions of environment have for many thousands of generations moulded the individuals of a given species of organism, and determined as each individual developed and grew "responsive" quantities in its parts (characters); yet, as Lamarck tells us, and as we know, there is in every individual born a potentiality which has *not* been extinguished. Change the normal conditions of the species in the case of a young individual taken to-day from the site where for thousands of generations its ancestors have responded in a perfectly defined way to the normal and defined conditions of environment; reduce the daily or the seasonal amount of solar radiation to which the individual is exposed; or remove the aqueous vapour from the atmosphere; or alter the chemical composition of the pabulum accessible; or force the individual to previously unaccustomed muscular effort or to new pressures and strains; and (as Lamarck bids us observe), in spite of all the long-continued response to the earlier normal specific conditions, the innate congenital potentiality shows itself. The individual under the new quantities of environing agencies, shows *new* responsive quantities in those parts of its structure concerned, new or "acquired" characters.

So far so good. What Lamarck next asks us to accept, as his "second law," seems not only to lack the support of experimental proof, but to be inconsistent with what has just preceded it. The new character, which is *ex hypothesi*, as was the old character (length, breadth, weight of a part) which it has replaced—a response to environment, a particular moulding or manipulation by incident forces of the potential congenital quality of the race—is, according to Lamarck, all of a sudden raised to extraordinary powers. The new or freshly-acquired character is declared by Lamarck and his adherents to be capable of transmission by generation; that is to say, it alters the potential character of the species. It is no longer a merely responsive or reactive character, determined quantitatively by quantitative conditions of the environment, but becomes fixed and incorporated in the potential of the race, so as to persist when other quantitative external conditions are substituted for those which originally determined it. In opposition to Lamarck, one must urge, in the first place, that this thing has never been shown experimentally to occur; and in the second place, there is no ground for holding its occurrence to be probable, but, on the contrary, strong reason for holding it to be improbable. Since the old character (length, breadth, weight) had not become fixed and congenital after many thousands of successive generations of individuals had developed it in response to environment, but gave place to a new character when new conditions operated on an individual (Lamarck's first law), why should we suppose that the new character is likely to become fixed after a much shorter time of responsive existence, or to escape the operation of the first law? Clearly there is no reason (so far as Lamarck's statement goes) for any such supposition, and the two so-called laws of Lamarck are at variance with one another. To push the matter further—in those cases in which experiment has been made, it has been found that a character acquired by an individual removed from the operation of a related condition normal to the race of which that individual is an example, is replaced by the old character when the old condition is restored in the environment of the offspring of that individual. No doubt I shall be challenged to

produce evidence confirming this last statement. I admit that carefully studied and conclusive instances are not very numerous, but I refer to such cases as the non-transmission (*a*) of plus or minus variation in pigment produced in individuals by greater or less exposure to sun-light; (*b*) the effects of dry or moist climate on individual plants; (*c*) the effects of change of diet on individual animals; (*d*) the effects of increased use of muscles in men and animals.

It seems that we are driven to the conclusion that the causes which have been active in producing changes the accumulation of which amounts to specific, generic and larger differences, must be causes which are able to act upon the potential congenital quality of the individual, and that there is no reason for associating the somewhat superficial and late responses or reactions of the parts of a growing individual to normal or abnormal forces of its environment with that more subtle and profound disturbance, which is permanent and affects the potential character of the germ, and more or less of all the germs derived from it.

At any rate this is the absolutely unanimous testimony of all those observers, in all countries and in all ages, who have been practically concerned (often with vast pecuniary interests at stake) in the production of relatively permanent new races of animals and plants. Breeders of horses, cattle, sheep and dogs, pigeon and poultry-fanciers, crop-growers, nurserymen, tulipomaniacs, and the like, have never in any single instance put the Lamarckian principle into practice. On the contrary, they laugh it to scorn. Not one of them ever produced a new race by moulding the parents. But, on the other hand, they *do* subject the selected parents to novel and disturbing conditions, to which the changed characters of the *offspring* (not of the parents) have no "responsive" relation; they cross-breed here and cross-breed there, until the "specific potential" is broken-down, and strange unlooked for varieties are born and grow up irrespective of normal or abnormal environment. From these congenital variations they select desired forms, and perpetuate them with perfect assurance and security.

For the present I see no evidence of a production of new races on the face of the earth, excepting by the method adopted by these men, viz. by the selection of congenital variations; such congenital variations being produced as the result of (but without any direct adaptational relation to) a disturbance of the material of the reproductive particles of both sexes; that disturbance being increased, if not determined, by changed environment of the parental organisms or the coupling of remote strains.

E. RAY LANKESTER.

Oxford, November 17.

The Present State of Physiological Research.

THE extracts given in NATURE of November 15, from an article by Prof. Max Verworn, of Jena, on "Modern Physiology," will serve to draw the attention of biologists to the reawakening of interest which is now evinced by many physiologists in regard to the fundamental phenomena exhibited by living things. As the opinion of physiologists is expressly invited in reference to the questions raised in this article, I venture to express my own as being in the main the same as that of Prof. Verworn.

It seems to me an obvious truism to say that the methods which can ensure a real advance in general biological knowledge must be those in which comparative physiology takes the lead. In my recent presidental address to the Liverpool Biological Society I urged the establishment of laboratories for the systematic study of the comparative physiology of the simpler organisms, the end in view not being the elucidation of the functions of organs with an *arrière pensée* as to their relation to man, but the examination of the activities for their own sake, since this inquiry forms the only means of approaching the mystery which enshrouds the essence of living existence.

The determination of the reactions of simple organisms to physical changes (stimuli), and the grouping of such resultant effects carried out systematically, form a line of physiological inquiry of transcendent importance, both because of its large scope and fundamental character, and because it opens the way towards the partial elucidation of the physiologist's real problem. This problem is the one involved in the question, to what extent all living phenomena are to be regarded as reaction phenomena? Are we, on the other hand, compelled to postulate the existence in every living thing of a *deus ex machina* which can, if it will, act independently of every physical stimulus, a so-called "vital force"?

Prof. Verworn is right in forcing upon the attention of physiologists the paramount necessity for work of this kind. I venture, however, to point out that he has not done justice to the judgment of his contemporary physiologists if, as I imagine, he has been led to infer from the character of the mass of current physiological work, that they do not realise the importance of such comparative physiology. But to realise the importance of an inquiry and to be able to carry that inquiry into effect, are unfortunately by no means identical positions.

There are undoubted obstacles to the latter, however ardently we may desire its fulfilment.

In the first place physiology is, in this country, more or less shackled by its position. It owes everything to medicine. Its laboratories are adjuncts to medical schools, its professors must take their share in the teaching in such schools, and this teaching is essentially connected with human physiology.

The debt, which as physiologists we owe to medicine, is one which we gratefully acknowledge, but even with the thanks on our lips we may be supremely conscious of the chains which still hang on and impede the debtor. It is this close relationship which, in my opinion, has served to accentuate the separation between physiology and the science of which it properly forms part, biology; a separation which is now almost a judicial one, and if unchecked may become an actual divorce. It is rare to fine a physiologist who has been highly trained in zoological investigation, and rarer still to find a zoologist who has attempted to perfect himself in the methods used in physiological laboratories. Yet the appropriate blend is essential for the advent of those comparative physiologists who alone can do full justice to the systematic inquiry now advocated.

Another difficulty in this country is undoubtedly due to the scanty pecuniary help afforded to scientific work which is neither technical nor directly concerned with what is regarded as the public good. Physiology, to-day, is maintained in Great Britain solely because it forms an essential part of a specialised technical education, that of the medical student; it is not maintained in order to inquire into the mystery of living things as such.

In order to adequately develop such an inquiry as this, it would be necessary to have a new department furnished with the equipment of both a zoological and a physiological laboratory, and with skilled workers who have leisure to prosecute their investigations. Since this means money, its full establishment may have to be postponed until that pious benefactor appears whose dawn even a Bodleian librarian has now anxiously to await.

Finally, I do not think the outlook is so discouraging as Prof. Verworn seems to believe, nor that "we are making no progress in physiology."

He admits that during the last twenty years we have attained to a precision in our experimental methods such as excites the astonishment of the uninitiated; and surely the mastery of method is the first step, and that an invaluable one, towards its future more fruitful employment. I do not imagine that even the systematic physiological investigation which he advocates, will involve the employment of new methods to the exclusion of old ones; it is the material which will be novel, not the entire experimental technique. Isolated instances of the application to simple excitable organisms, of such physiological methods as have been employed in elaborate detail for the investigation of muscle, nerve, &c., are well known to us all, and to no one better than Prof. Verworn; the real desideratum is surely that the instances should be no longer isolated, but form part of a broad systematic inquiry.

FRANCIS GOTCH.

University College, Liverpool, November 17.

Wilde's Theory of the Secular Variation of Terrestrial Magnetism.

MR. WILDE's reply in NATURE of October 11 to my letter of criticism in the same of August 9, with respect to his communication to the Royal Society, contained in the *Proceedings* for March 1894, has just come to my attention.

As the letter consists entirely in an attempt to show the inaccuracy and unreliability of my statements with respect to the inclination-observations made at St. Helena,

behoves me to confine my own remarks to this matter. In order that my explanations may be understood, the table previously given is here reproduced.

No.	Date.	Observer.	Observed inclination.	Wilde's inclination.	Observation − Theory.
			°	°	°
1	1700	Hansteen Chart	2 S.	3·9 N.	−5·9
2	1754·3	La Caille	9·00	0·5 S.	−8·5
3	1771·4	Ekeberg	13·00	3·5	−9·5
4	1775·4	Cook	11·42	4·0	−7·4
5	1780	Hansteen Chart	10·5	5·1	−5·4
6	1825·0	Duperrey	14·93	14·7	−0·2
7	1840·1	Ross	18·27	18·5	+0·2
8	1842·3	Belcher	17·00	19·0	+2·0
9	1846·8	Smyth	19·39	20·5	+1·1
10	1890·1	U.S.C. & G.S	29·65	33·8	+4·1
11	1890·1	,,	31·18 S.	33·8	+2·6

This table is in every respect the same as originally given with the exception of the value for 1700, the erroneous value of 11°·5 S. having been given instead of 2° S. Owing to my sojourn in Europe being but a temporary one, I have not with me all my data, and so cannot ascertain definitely how this error crept into my table. My original scaling was probably 1·5 S., which by a copying blunder may have been converted into 11·5. This, however, I cannot control now. It would have been a most natural inference on Mr. Wilde's part if he had ascribed this to the "printer's devil," all the more so as no use whatever was made in the text of my communication with this value. On account of so apparent an error he casts a slur upon my trustworthiness in general in regard to terrestrial magnetic matters. Such a poor method of argument reveals the weakness of his position.

Furthermore, with reference to this table, Mr. Wilde says: "I regret to observe that L. A. Bauer, in his intolerance of the magnetarium results, has inserted in his table guesses of his own for observations, which are very wide of the truth." I can find no excuse whatever for this statement. Mr. Wilde has acknowledged that he possesses a copy of Hansteen's "Magnetisms der Erde." Let him turn to Tafel II.: "Neigung der Magnetnadel," p. 36, and he will find the following observations given for St. Helena:—

Observer.	Date.	Inclination.
		° ′
De la Caille . .	April 10, 1754	9 0 S.
Ekeberg. . . .	May 19, 1771	13 0 S.
Cook	May 17, 1775	11 25 S.

Position assigned by Hansteen: latitude 15° 55′ S., longitude 11° 52′ E. of Ferro, or 354° 12′ E. of Greenwich. By inspecting the table given above it will be seen that these form Observations Nos. 2, 3, and 4. No. 5, as stated, is taken from Hansteen's Chart for 1780. Mr. Wilde does not appear to question this value, nor the remaining ones, which can be easily found in Sabine's "Contributions to Terrestrial Magnetism." He will, furthermore, find that Hansteen had so much faith in the early observations, which Mr. Wilde insinuates are untrustworthy, that in 1857 he made use of all the observations known up to that time, viz. Nos. 2, 3, 4, 6, 7, 8, and 9, for the establishment of a periodic formula representing the secular variation of the inclination during this epoch. These investigations of Hansteen's can be found in "Den magnetiske Inclinations Forandringer i den nordlige og sydlige Halvkugle af Christopher Hansteen," Copenhagen, 1857, −4°. Hansteen, by a least square adjustment of the observations named, derived the following interpolation formula:—

$$i = -13°58'·455 - 5'·44405\,(t-1800) - 0'·001013\,(t-1800)^2.$$

i denotes the inclination at the time t, south inclination being reckoned as minus. This formula at the utmost should not be used more than ten years prior to 1754, nor ten years later than 1846. The following table shows how the values computed with this formula agree with observation.

Date.	Observed inclination.	Computed inclination.	O. − C.
	°	°	°
1754·28	− 9·00	− 9·85	+0·85
1771·38	−13·00	−11·38	−1·62
1775·38	−11·42	−11·77	+0·35
1824·96	−14·93	−16·26	+1·33
1840·10	−18·27	−17·66	−0·61
1842·35	−17·00	−17·87	+0·87
1846·79	−19·39	−18·28	−1·11

From this comparison it will be seen that the formula represents the observations fairly well. Let us compute then with it what the inclination would be in 1747. We obtain −9°·2. Now Mr. Wilde's magnetarium has given us for this date the value 0°·0. Hence there is an outstanding difference of about 9°, which he has made no attempt to explain otherwise than by insinuating that I have put "guesses" in my table, or that the observations are untrustworthy. The burden of the proof that the observations are not trustworthy, rests with Mr. Wilde. Anyone, who has made any endeavour to familiarise himself with the literature of the subject of terrestrial magnetism, will know that it is an old story for theorists to characterise observations as doubtful if they do not happen to agree with their theory. I am willing to admit that the early inclinations in such a locally disturbed region as St. Helena, perhaps, cannot be depended upon nearer than to 2° or 3°; but, if Mr. Wilde will pardon my scepticism, I do not believe that it is possible for him to reproduce any inclination with his magnetarium that can be relied upon even to this extent. Hence, it is fair for me to compare the magnetarium results with that of observations (so long as the latter have not been overthrown) without consideration of the probable error of either result.

Mr. Wilde appears to have thought his position proven when he found that I made an error with respect to my first value. But even with the value as given by him, the outstanding difference is 5°–6°, with which he appears perfectly satisfied. If he will permit me a probable error as large as he permits himself in the establishment of his theory with his magnetarium, I can supply him with a dozen periods that will satisfy observations as well as his magnetarium. I would like to refer him to a recent attempt by Dr. Felgentraeger, who endeavours to prove the universality of the secular period by establishing periodic formulæ upon the basis of most carefully collected material. He deduces a period of 477 years—instead of Mr. Wilde's 960—upon the basis of the declination observations made at London 1580–1882, and Paris 1541–1890.[1] Adopting this period he found that he could represent exceedingly well the observations made at London, Paris, Rome, Clausthal, Chambersburg (U.S.A.), Rio de Janeiro, Cape of Good Hope, and Cape Comorin. Here we have a more extensive comparison than Mr. Wilde has given us, and we find a better agreement with observations with a period one-half of his! In this brief communication I cannot set forth my own position with respect to the secular-variation period. I hope to present Mr. Wilde with a copy of my investigations some time in December.

With respect to my opinion of the magnetarium in particular, I may say that my criticisms made thus far have applied solely to the theory as evolved from the magnetarium results, and do not touch the magnetarium as a valuable instrument of research. Indeed, I think much good can be accomplished with it. Mr. Wilde has made a most laudable attempt to reproduce mechanically the complex phenomena of terrestrial magnetism, and if the achievements with his ingenious mechanism had not received the publicity they did, or had been properly interpreted, my criticisms would never have been made. That he has not succeeded in giving us a better representation is no fault of his, but owing to the complexity of the phenomena.

The fact that Mr. Wilde has succeeded, by an arbitrary distribution of magnetic matter in his magnetarium, in representing the *distribution* of terrestrial magnetism for the year 1880 apparently so well, is no proof of his secular-variation theory or his period, which plays no part in determining the distribution. Nor is the fact that with *his* distribution of magnetic matter he gets a good representation a proof that *that* is the actual distri-

[1] Dr. W. Felgentraeger: Die längste nachweisbare Säculare Periode der erdmagnetischen Elemente. Teil 1: Deklination. Inaug. Diss. Universität zu Göttingen, Göttingen, 1892. Buchdruckerei von Louis Hofer.

bution prevailing in the earth. It can be demonstrated as a mathematical fact that in the absence of terrestrial magnetic observations within and without the earth's surface, an infinite number of different distributions is possible that will satisfy the effects observed on the surface alone.

In conclusion, it is my duty to make one more explanation. Mr. Wilde understood from my first letter that I am still in the employ of the U.S. Coast and Geodetic Survey. In view of the fact stated by him, that he sent, at considerable trouble and expense to himself, a duplicate of his magnetarium to the "Survey," and, hence, it might appear that it was somewhat discourteous in a member of the "Survey" to thus criticise him publicly, I may say that I severed my connection with the Survey two years ago, and that my criticisms have been made without any knowledge whatsoever of what has been accomplished with it by the "Survey."

Friedenau bei Berlin, October 31. L. A. BAUER.

Boltzmann's Minimum Theorem.

MR. CULVERWELL'S letter of October 25 ought to have received a much earlier answer. That it did not do so was owing to purely accidental circumstances which I very much regret.

In that letter Mr. Culverwell criticises my treatment of Boltzmann's familiar proposition concerning the properties of the H function on the following grounds :—

(1) The choice of the generalised coordinates. In investigating the circumstances of a collision or an encounter between two systems of molecules of m and n degrees of freedom respectively, he sees a difficulty in my choice of the coordinates as $Q_1, Q_2 \ldots Q_m, q_1, q_2 \ldots q_n$, where ($q_n = a$) determines a collision, or encounter. But supposing the requisite number of degrees of freedom to be secured, the choice of the independent variables is surely quite optional. I had myself assumed this as self-evident, perhaps too hastily, but at any rate Mr. G. H. Bryan has placed this proposition beyond doubt, in the exhaustive report submitted by him to the British Association last August. Take for example sets of plane circular disks moving amongst each other in their own plane; here each pair of disks constitutes a material system, whose position is completely determined (assuming the orientation of each separate disk to be indifferent) by the following four variables, viz. the two coordinates of the centre of one disk of the pair, the distance ρ between their centres, and the inclination of that distance to a line fixed in the plane; this third variable ρ is the q_n of my proposition.

(2) Mr. Culverwell objects that the general Boltzmann proposition $\left(\frac{dH}{dt}\right.$, always negative unless $\left. Ff = F'f'\right)$, or H a minimum for one, and one distribution only, cannot be true, because if a system were started from any initial configuration (P, Q), and after the time t arrived at the configuration (p, q) and the definite integral H were evaluated in these two configurations, the proposition asserts that the second H must be less than the first H, or $H_t < H_o$, whence it would follow by the same proposition that if at the end of the time t each velocity component were reversed, the H_{2t} must be less than H_t, and this, doubtless, I do assert. But Mr. Culverwell maintains that such an assertion is obviously untrue because at the end of the second interval t the system has returned to its *original* condition, and therefore H_{2t} must be the same as H_o, and to this proposition I demur.

Doubtless when a material system in a field of any conservative forces is started from any initial position and velocities, arrives after a time t at another position and with other velocities, and here has each velocity reversed, it is true that at the end of the next interval t it will be in its initial position, with each velocity component reversed; but it remains to be proved, and cannot be asserted *à priori*, that the H_{2t} is equal to H_o, and the only proposition available for the investigation is this very proposition of Boltzmann's, which proves that H_{2t} will be less than H_t, and therefore less than H_o.

Finally, Mr. Culverwell inquires, somewhat despondently, if anyone will point out the use of the H function, and what is proved by it.

I have already said in my second edition, that the proposition is not of my invention, and therefore that I have no claim to answer this question with any authority, but to my own mind the proposition appears certainly to clear away one obvious difficulty. Without the aid of this proposition we are enabled to assert that if F (p, q) were a function of the co ordinates and momenta of a molecule such that in the absence of encounters *with other molecules*, F remains constant for all time, then the form of F satisfying the condition $Ff = F'f'$ must render F $(p, q)\ dp\ dq$ a *permanent* law of distribution, and therefore if we can assert that F (p, q) must *of necessity* be of the form F (E) (E sum of potential and kinetic energy), then the e^{-hE} law of distribution is certainly a permanent law (neglecting, *i.e.*, all but binary encounters); but in the absence of this proposition, we cannot assert that the $Ff = F'f'$ is *necessary* as well as sufficient, because we cannot insist upon the necessity of an exact compensation in the passage from the $p\ q$ to the $p'\ q'$ state, and conversely, taking place at *each separate* encounter. The H proposition, therefore, removes this element of uncertainty, and reduces the question to that of the F (E) restriction, because it proves that unless $Ff = F'f'$ for *each pair* of encountering molecules, H and therefore F and f must be a function of the time. As I have already asked for a disproportionate share of your space, I will not enter upon the question of the F (E) restriction now.

H. W. WATSON.

I DID not exactly, as Mr. Burbury suggests, question Boltzmann's minimum theorem, but only the pages thereon in Dr. Watson's "Kinetic Theory of Gases." Indeed, I said that though I had not seen Boltzmann's proof, I supposed it to be all right.

It appears from Mr. Burbury's letter that in order to prove the theorem, even for the simple case of perfectly hard and elastic spheres, some amount of assumption as to an average state having been already attained must first be made, and of course the *à priori* reasoning in my letter is not applicable to such a theorem. Mr. Burbury's letter is exactly the kind of letter I hoped to elicit, and if he can say what assumption in a generalised system will replace the assumption of equal distribution of velocities in different direction in a system of hard spheres, he will clear up the whole difficulty. Unfortunately, the case with which he deals is one in which the error-law is known from other considerations to be the only permanent state.

I observe that Mr. Bryan, in his British Association Report, quotes the oversight I pointed out in Dr. Watson's proof, without making any criticism on it.

EDWD. P. CULVERWELL.

Trinity College, Dublin, November 24.

The Alleged Absoluteness of Motions of Rotation.

PROF. GREENHILL, in his review of Mach's "Science of Mechanics" (NATURE, November 15), writes as if he disapproved of that author's not accepting "Newton's distinction between the relativity of motion of translation and the absoluteness of motion of rotation." He appears to think that Mach would have obtained more insight into this distinction from a study of Maxwell's "Matter and Motion." It might more truly be said that Maxwell would have profited by a perusal of Mach's book The latter finally refutes the paradox contained in Newton's statement, and supported by Maxwell, and by so doing renders a great service to Mechanical Science. He has disposed once and for all of "absolute rotation." It is high time that writers on Mechanics should revise the preliminaries of their science so as to state their results in terms of relative motion, whether of translation or rotation. This has been partially done by Maxwell, and a further step has now been taken by Mach. It is unfortunate that the reviewer in drawing attention to this part of the book should have preferred to stand by the prejudice he owes to Newton and Maxwell when he might have done something to hasten its abandonment.

A. E. H. LOVE.

St. John's College, Cambridge, November 20.

MACH says truly (p. 237) "that precisely the apparently simplest mechanical principles are of a very complicated character; that these principles are founded on uncompleted experiences, which can never be fully completed," &c.

The modern student of theoretical mechanics is in a dilemma;

either he must accept the complete idea of the relativity of all motion, of rotation as well as of translation, as professed by Milton, Mach, and Mr. Love; or else he must follow Newton, Maxwell, and the German writers Streintz and Lange (attacked by Mach in Appendix iv.), and distinguish between the relativity of the motion of translation and the absoluteness of rotation. Euler, it appears, was a waverer, and according to Lange never arrived at any settled and intelligible opinion upon the subject. The first theory appears more analogically complete, but introduces unnecessary complication at an early stage; and stronger arguments than those of Mach, and others that I have yet met with, will be required to convert me to their side of the question.

A. G. GREENHILL.

November 26.

Science Teaching in Schools.

IN the discussion on the teaching of science, and in the schemes put forward for reorganising this teaching, mathematics has so far been left out of consideration.

At present mathematics is taught for its own educational value, which has been traditional since the time of Plato; only in modern times has its great practical value been recognised. The teaching in schools takes little account, however, of the applications of mathematics, and whatever Prof. Greenhill may say (in his review of Prof. Mach's excellent book), there is still wanting complete harmony between those two points of view; not perhaps in the higher branches of the subject and its applications, but certainly in school teaching.

Boys, and girls too, in public schools are taught the elements of mathematics as if all were expected to become mathematicians, and the practical side is kept out of view. In the modern, or science side, which has been introduced at many schools, one finds too often chiefly those boys who show no talent either for classics or mathematics. Many of these have made little or no progress in Euclid; they cannot grasp the altogether abstract notions and symbols of algebra, and they therefore never come near trigonometry. But they are expected to understand the elements of chemistry, mechanics and physics; and it is instructive to find that they very often do understand a good deal of what is taught under these headings.

Now none of these subjects can be accurately taught—and inaccurate teaching is worse than waste of time—without the introduction of mathematical reasoning. Here we are in a vicious circle: the boys are considered incapable of learning mathematics, and therefore mechanics and physics have to be taught without any more than the most elementary notions of geometry and algebra; hence not much progress can be made.

In my opinion the order of procedure might be reversed. Mathematics might be taught through experimental science. If the boys themselves make, as they should do, experiments where they perform actual measurements, they will learn there are certain laws connecting various quantities; they will see that such laws can be expressed in simple symbols, and they will thus grasp in the concrete form the meaning of a formula or an equation which in the abstract form of pure mathematics remained a mystery to them.

Mathematics could in this manner be made very much easier and more interesting to the majority of boys. Geometry can be treated to a very great extent experimentally by aid of geometrical drawing and a development of the Kindergarten methods; the abstract logic of Euclid can then follow, or it can be treated at the same time.

Trigonometry need not be at once as fully gone into as is generally done, but the definitions of sine, cosine, &c., as names for certain ratios, can be easily and early introduced and made use of at once in mechanics or physics. Here also special experiments may easily be devised where measurements of angles or lines are made, and lines and angles calculated.

To explain fully what I mean I should require a great deal of space; in fact it would be almost necessary to draw up a distinct syllabus for a course on the above lines, or to give at least a great number of examples.

At present I wish only to urge that, while many attempts are being made to improve science teaching, and with it technical education, mathematics should be included, and to express my opinion that this science also allows of experimental treatment.

November 19. O. HENRICI.

MR. CRUMP (*vide* p. 56) though adopting a critical form and tone, really endorses the grounds of my suggestion that the Science and Art Department should dissever itself by an age limit from school science. He is inclined to be especially severe upon the defects of the Government examinations because they are controlled by scientific men, and to excuse the proper school examining boards because they have—according to Mr. Crump—attempted to examine in science without any qualification to do so. But I fail to see why eminent scientific men should be expected to be experts in elementary science teaching, any more than distinguished *littérateurs*, in the art of teaching to read, and it seems to me—in spite of Mr. Crump's "absolute" denial—that examining boards, neither professedly literary nor scientific but professedly educational, are more to blame in following and abetting the Department's premium upon text-book cramming. The fact remains that the London Matriculate ignores practical teaching of any kind, and that the "practical chemistry" of the Locals and College of Preceptors is essentially the same test-tube analysis as the South Kensington examination. Anyone who knows the London Matriculation examination—witness Miss Heath's concluding remark—will appreciate the quiet humour of Mr. Crump's allusion to it as "awakening and developing the powers of observation and reasoning."

H. G. WELLS.

The Explosion of a Mixture of Acetylene and Oxygen.

WITH reference to your note in last week's NATURE, I may say that, whilst the thanks of chemists, and particularly of those whose duty it is to perform lecture-experiments, are due to Prof. Lothar Meyer for once more drawing attention to the dangerously explosive nature of mixtures of acetylene and oxygen, it may be assumed that the facts already known concerning acetylene account sufficiently well for the great violence of the explosion, and hence for the circumstance that the mixture will shatter even the open cylinder in which it is detonated. What M. Berthelot terms the molecular rapidity of the reaction, as distinguished from the rapidity of propagation, in the case of mixtures of acetylene and oxygen is very high. The heat of the reaction, too, is nearly five times as much as in the cases of electrolytic gas and of carbonic oxide, and more than twice as much as that of methane. It is slightly exceeded by that of ethylene, but, on the other hand, the theoretical temperature of the change with acetylene is enormously greater than in the case of any other explosive mixture of gases. The temperature, too, required to initiate the change is, as Prof. Lothar Meyer showed indirectly some ten years ago, much lower in the case of acetylene than in that of the other gaseous mixtures of which he speaks. All the conditions tend to make the duration of the reaction so nearly instantaneous that the initial pressure cannot be far removed from the theoretical pressure, and this is sufficient to smash a much stronger envelope than a glass cylinder, even if the "tamping" be nothing more than the air. Everything we know about acetylene combines to show that it is extremely "sensitive" as an explosive, and that in this respect, as in its destructive action, it resembles mercuric fulminate.

T. E. THORPE.

"Newth's Inorganic Chemistry."

THERE are one or two points in Mr. Pattison Muir's review, upon which I should like to be allowed to say a few words.

Criticising the general plan of the book he says:

"It seems to me that the method of the author is radically wrong. Descriptive statements of facts ought surely, neither to precede, nor to follow, but to accompany the reasoning on these facts whereby general principles are gained."

It is not easy to see how the *descriptive statements of facts*, and the *reasoning on these facts* are to be printed in a book at all, unless one either precedes or follows the other. I can only suppose that my reviewer means, that such theoretical and other considerations as I have included in part i., and have called "introductory outlines," should in his opinion not be collected together either at the beginning or at the end of the book, but should be sprinkled among the descriptive chapters. It seems to me that the plan I have adopted, besides being a

more orderly arrangement, and one more convenient for reference, is also the one that best enables the student to study the subject in the way advocated by my reviewer; and in order to impress upon the student that the study of descriptive facts should *accompany* the study of the reasoning on these facts, he is directed to "slowly and carefully" read part i. *while he is studying the descriptive chapters.* I venture to think also, that this method tends far less to "perpetuate the vicious and unreal distinction between chemistry and chemical philosophy" than that of obliging the student to gain his information of facts from one book, and his knowledge of theory from another.

Commenting upon the fact that part ii. is devoted to the study of four typical elements, Mr. Muir says:

"But hydrogen, oxygen, nitrogen, and carbon are not treated as typical elements; they are not compared and contrasted with other elements."

This criticism is not true. Chapter ii. of part iii. is prefaced with a short general account of the elements oxygen, sulphur, selenium, tellurium, in which the typical element oxygen is compared and contrasted with its *confrères*. Chapter iii. is prefaced with a similar brief sketch of the elements nitrogen, phosphorus, arsenic, antimony, bismuth, wherein the typical element nitrogen is compared and contrasted with the others of the group; and similarly at the beginning of chapter ix. the typical element carbon is compared and contrasted with silicon, germanium, tin and lead.

My reviewer is good enough to say: "The descriptions in this book of the members of each group of elements seem to me to be exceedingly well done; many portions of the chapters treating of principles and theories . . . are admirable." And again, a few lines further on: "The purely descriptive portions of the work are often extremely good, as far as they go. The facts, or rather half-facts, are stated in a clear and orderly way."

I am a little curious to know what *half-facts* are; and whether if such things can be, it would be possible to state them "in a clear and orderly way." If my reviewer merely means that there are so many more facts known than I have stated, that roughly speaking it may be said that I have only described one half of the known facts, I can only reply that I have endeavoured to select "from the overwhelming burden of so-called facts" such as seemed to me to be most important for the student, and which could be conveniently included within the limits of a small text-book.

Mr. Muir finds fault with my book because he does not discover in it "some fair and fitly fashioned building," which he says "ought to rise on this broad superstructure." I regret that this objection has not been stated in rather more explicit terms; I have tried to understand it, but cannot—perhaps it is poetical. In ordinary language one does not speak of a building as rising upon a superstructure. In no text-book of chemistry with which I am acquainted, is any trace of such a phantom edifice to be found, and it is sincerely to be hoped, that when the Joshua appears, who by raising such a "fitly fashioned building" shall "rescue chemistry from the overwhelming burden of so-called facts beneath which the science is in danger of being buried," he will choose some more suitable vehicle for making his views known to the scientific world than that of an elementary text-book on inorganic chemistry.

G. S. NEWTH.

I STILL hold that Mr. Newth's method is radically wrong. I admit it is not easy to make the descriptive statements of chemical facts accompany the reasoning on these facts; but although not easy it can be done.

As regards Mr. Newth's treatment of the four typical elements, hydrogen, oxygen, nitrogen, and carbon, I can only repeat that the comparisons and contrasts made between these elements and those of which they are representative are, in my opinion, worth very little.

I cannot enter into a discussion of the meaning of the term "half-fact"; but I can assure Mr. Newth that in saying he had stated "half-facts in a clear and orderly way," I did not mean to say he had stated about half of the known facts and omitted the rest. It is characteristic of half-facts that they are very amenable to clear and orderly arrangement.

When I spoke of "some fair and fitly fashioned building" rising "on this broad superstructure," of course I should have written "broad substructure." I am much obliged to Mr. Newth for pointing out this stupid slip.

I thought Joshua was more concerned with demolishing towns than with raising buildings; but my Hebrew history is a little rusty. M. M. PATTISON MUIR.

Cambridge, Nov. 21.

Singing Water-Pipes.

AT Oxford, Prof. Osborne Reynolds showed an interesting case of sound in water. There is another familiar effect, of which he has probably given the reason, but it does not seem to be commonly known.

A little while back there was a clear steady note carried through my house by the water-pipes, a note of the middle octave of the quality of an organ diapason pipe. When the source was found, it was easy to change the note through the octave. The music arose as often as the scullery tap was turned on, and lasted so long as the water was running. The tap was worn, and the flow of water kept up a rapid tapping of the loose part, just as in Trevelyan's Rocker.

The singing is sometimes heard after a tap is turned off. This happens because the ball-tap of a cistern has thus been left running. W. B. CROFT.

Winchester College, November 26.

An Aurora on November 23.

STEPPING out of doors to-night, November 23, at 7.30, I was surprised to see the whole northern sky filled with luminous mist, so clear that our shadows were dimly observed on the shining surface of the wet highway. There were few tremulous motions, but the light clouds advanced southwards in great patches. For a while the planet Jupiter shone to the east of the luminous haze. Then the mist passed over Jupiter, who shone, however, with nearly its wonted splendour until a great detached belt hung between Jupiter and Pleiades, over to the south-west horizon.

The Milky Way became obscured as the haze passed right over our heads. By eight o'clock the detached luminous belt, which was not uniform, but in patches, had reached the planet Mars. Neither was the light in the north uniform, but here and there were clear spaces. By 8.10 the aurora was much dimmer. By 8.30 there was no luminosity except in the north, between the Great Bear and the horizon. J. SHAW.

Tynron, Dumfriesshire.

A Snake "Playing 'Possum."

A PUFFING adder, *Heterodon platyrhinus*, caught by the writer in May 1894, exhibited a most curious instance of feigned death which may be worthy of record.

The snake when discovered at first tried to escape, but on being captured it turned on itself with mouth wide open, head thrown back sharply, and tongue limp and protruding. The mouth remained open thus to its fullest extent, while the head and upper part of the body threshed violently from side to side for a few times, and then his snakeship rolled over on his back, and after a few convulsive movements became apparently lifeless. The body was then quite limp, and remained in whatever position it was placed, providing the snake was on his back, but when turned over in the proper position, he immediately rolled back by an almost imperceptible muscular contraction. When struck lightly, pinched or held up by the tail, there was very slight resistance. He continued in this state for about half an hour, when no attention having been paid to him, he resumed his normal position. A little teasing caused a repetition of this performance a number of times afterwards, and it did not vary in any essential particular. It would be interesting to know whether this is a ruse common to individuals of this species, and if so whether it is confined to them alone.

L. C. JONES.

The Soaking of Seeds.

IN reply to Mr. Alfred W. Bennett's inquiry as to the soaking of seeds in milk before sowing, it may interest him to learn that in book iii. section v. of his "Deipnosophists," Theophrastus is quoted by Athenæus as saying that "cucumbers contain a more agreeable and wholesome juice if the seed be steeped in milk or mead before it is sown," and that "plants come up quicker if they are steeped in water or milk before they are put in the ground." P. C. GLUBB.

Pendean, Liskeard, November 13.

HISTORY OF ENCKE'S COMET.

HISTORICALLY, Encke's comet, which has recently come into view again, stands next in interest to Halley's. The history of the latter can be carried back much farther than that of any other comet. It was indeed conjectured that the one (the first telescopically discovered comet, by Kirch) which made so near an approach to the sun in 1680, and in reference to which Newton first applied his principle of universal gravitation to the motions of these bodies, was identical with comets seen at intervals of about 575 years before, and Gibbon (not exactly an astronomical authority, who recommends others to study Newton and Halley on the question) devotes a section of his forty-third chapter to the supposed history of these early appearances (two of them in mythical times), concluding with the remark that the calculations with regard to it might perhaps in the year 2355 "be verified by the astronomers of some future capital in the Siberian or American wilderness," little thinking how many splendid telescopes would be employed on the study of the heavens in the "far west," before a century had elapsed from his own death. There was then, in 1794, no observatory on any part of the American continent. But it is now known that the period of the comet of 1680 amounts not only to hundreds, but to thousands of years; and one of the supposed previous appearances, that in the reign of the Emperor Justinian, was in all probability a return of the smaller or less conspicuous comet which appeared in 1682, and at the next return in 1758–9 acquired the name of Halley's comet, because that eminent astronomer had confidently predicted its return at that date, calling upon posterity to notice that the prediction had been made by an Englishman. He recognised its identity with comets observed in 1531 and 1607, by a comparison of the orbits calculated for each, and considered from the similarity of period, that the fine comet of 1456 was also probably an earlier appearance of the same. Later investigations, and the accessibility of Chinese records, have shown since his time that successive appearances of this body can be traced with very great probability to a date before the Christian era, our distinguished countryman, Dr. Hind, having taken a leading part in these calculations.

Of the subsequent observations of this comet in 1835, this is not the place to speak, nor of the full expectation astronomers then living will entertain of seeing it again in 1910, and applying the new methods of analysis to it, thereby obtaining information respecting its constitution, which was beyond the wildest flights of imagination at its last appearance. For our present subject is a comet which acquired the name by which it is now universally known as a fitting meed of honour to an astronomer who worthily presided over the then new observatory at Berlin within our own recollection. Many comets have since that time returned according to prediction; but when Encke announced that the small one discovered by Pons at Marseilles on November 26, 1818, was identical with the discovery of Méchain in 1786, of Miss Herschel in 1795, and of Thulis in 1805, no predicted return of any comet but Halley's had ever taken place, though two predictions had been made, by himself and by Bessel respectively, of the returns of comets observed in 1812 and 1815, which duly came to pass in 1883 and 1887, the periods of these being nearly as long as that of Halley's. The remarkable point about Encke's comet was the extreme shortness of its period, amounting only to 1212 days, or three years and about four months. It was therefore concluded that it would reappear in 1822; true to prediction it did then appear, but from its situation in the heavens was visible only in the southern hemisphere, which then possessed only *one* observatory, that established (but which has long ceased to exist) by Sir Thomas Brisbane at Paramatta, New South Wales, where the comet was rediscovered by Rümker on June 2. The next appearance took place in the autumn of 1825, when the comet was observed in this hemisphere, and since that time it has never failed to be observed at the calculated epochs. Encke did not desert it after he had determined its period in 1819, but, following up its motions with accuracy, was led to notice a remarkable continuous shortening of the period by a fraction of a day at each return. The question had before his time been started whether a medium might be diffused through the solar system which, though insufficient to affect the motions of the planets, would produce appreciable effects upon those of comets, composed as they must be of matter in a state of great rarity. Here was a case which seemed to settle the question in the affirmative. Encke's calculations showing that the diminution in the observed length of the period was such as might well be caused by the action of such a resisting medium checking the onward motion of the comet, which would bring it a little nearer to the sun at each return, and thus shorten both the orbit of revolution and the period of time in which it was accomplished. The difficulty remained how to explain the fact that no such effect was perceptible in the motions of any other comet; a difficulty which the lapse of time has not removed, for though in one other case (that of a comet known as Winnecke's) a similar effect was for a while thought to be noticed, further investigation showed that this view could not be sustained. Encke, however, to the end of his life (he died in 1865) was able to trace the above continuous effect in the motion of his own comet, the period of which was then 1210·2 days, or 1·6 days shorter than it had been in 1819. But, strangely enough, soon afterwards, the amount of retardation was reduced by about one half, at which it has remained from 1868 to the present time. Must the resisting-medium theory be modified, or must it be altogether abandoned and some other cause be sought for the retardation in question? Prof. Young suggests a regularly-recurring encounter with a cloud of meteoric matter.

When nearest the sun, Encke's comet is at very nearly the same distance from him as the planet Mercury. When farthest from him, it is in the zone of small planets (nearly four hundred of which are now known), revolving between the orbits of Mars and Jupiter. May the attraction of some of these have something to do with the effect above referred to? Small as is the mass of most of the tiny bodies in question, it is possible that at certain times some of them may act together and produce a cumulative and appreciable effect. Of great value to astronomy has been the position of Encke's comet at the other extremity of its orbit, in perihelion. Before its discovery, the mass of Mercury had been rather a matter of conjectural inference than of actual calculation, that planet having no satellite the motions of which would be affected by its attraction. But at certain returns, the comet of which we are treating, made very near approaches to the planet, and the effects produced on these occasions have enabled astronomers to obtain determinations of the mass of the planet as accurate, or nearly so, as those determined for the larger planets which have satellites. The first of these near approaches since the comet's discovery took place in 1835; the last at the most recent return, in 1891.

We now come to the physical appearance of Encke's comet. It has on some occasions, when nearest the Earth, been just visible to the naked eye, particularly in the autumns of 1828 and 1848. After Miss Herschel had detected it (supposed to be a new comet), at its return in 1795, her brother, Sir William Herschel, observed it on November 8, and noticed it the following day pass centrally over a star of the twelfth magnitude without obscuring it, whence he concluded that the comet "is evidently nothing but what may be called a collection of vapours." Maskelyne, who observed it at

Greenwich a few nights afterwards (Bode had in the meantime, in company with an amateur astronomical friend, detected it at Berlin on November 11, four days after Miss Herschel's discovery at Slough), contested this view on the ground that the nucleus might be situated not in the apparent centre of the comet. And this indeed would seem to be the case; the general appearance of the comet, when seen under the most favourable circumstances, being that of a slightly oval vaporous mass, with a small ill-defined nucleus eccentrically situated within the coma. In 1848, towards the end of September, a faint brush of light was noticed by Prof. Bode, extending from the more condensed part of this towards the sun; and a few weeks afterwards a tail, between one and two degrees in length, was seen on the other side, *i.e.* the normal position of a comet's tail. Late in the month of November in the same year, it may be remarked, the comet made one of its very near approaches to Mercury, coming within the distance 0·038 of the Earth's mean distance from the sun, or about three and a half millions of miles. The return of 1871 was a noteworthy one in several respects; and particularly for the remarkable fan-like appearance which the coma presented in November and December.

The apparent contraction of a comet's bulk as it approaches the sun, and dilatation of it again when receding from him, which has been manifested in several of these bodies, has been especially marked in the case of Encke's, the visible diameter at perihelion being not equal to the twentieth part of what it is about the time when the comet first comes into view. The most probable cause of this would seem to be that suggested by Sir John Herschel, which would make it rather apparent than real, namely, that near the sun a part of the cometary matter becomes invisible by evaporation, just as a cloud of fog might be.

In 1871 the spectrum of this comet was examined by Prof. Young, and found to consist of three bright bands, of which the central one was the most prominent; they were somewhat sharply defined on the least refrangible side, whilst on the other they were diffused. "Of a continuous spectrum there was no trace, and the spectrum was the same from every part of the comet." But in 1881 a faint continuous spectrum was detected by Prof. Tacchini, so that the result of spectrum analysis applied to this comet would seem to be essentially the same as that obtained from the great majority of comets of which the light has been examined in this way.

At the last appearance of Encke's comet, in 1891, it was first seen by Prof. Barnard at the Lick Observatory in California, at the beginning of August, and passed its perihelion on October 18. At the present return it was detected at the Nice Observatory on October 31, in the constellation Pegasus; and Dr. Max Wolf found it registered on a photographic plate taken by him the same evening at Heidelberg. As on several previous occasions, its ephemeris has been calculated by Dr. Backlund, of Pulkowa; and it is matter of regret to notice his accompanying announcement that this is the last occasion on which he will be able to undertake it. The earlier portion of this ephemeris was given in NATURE last week.

W. T. LYNN.

PROGRESS OF THE CATARACT CONSTRUCTION COMPANY'S WORKS AT NIAGARA.

THE general scheme of the Niagara Falls Power Company has already been described in these columns (NATURE, vol. xlix. p. 482). We understand that the great power house is now complete, and the foundations ready for the three great 5000-horse power dynamos which have been constructed by the Westinghouse Company. The turbines and vertical shafts up to the floor of the power house have long been in place, and the dynamos may now be shipped any day. They have already been revolved in the shops at full speed. Our readers will remember that there is no gearing. The dynamos are on a vertical shaft, and the revolving fields are external to the armature, forming a sort of bell-cover to it, with the poles pointing radially inwards. This was the only design which Prof. Forbes could make to fulfil the requirements of the turbine designers as to maximum weight and minimum fly-wheel effect to be allowed. It gives a splendid mechanical construction, as the revolving pole-pieces and coils are retained in place against the centrifugal forces by the nickel-steel ring which forms the yoke.

The first place to be supplied with current is the aluminium works of the Pittsburg Reduction Company. To convert the two-phase, 2000 volt alternating current into a continuous current of 160 volts at these works, 2500 feet from the power house, transformers are there used, and the low pressure alternating current in two phases is supplied to commutating machines. This is a new departure of great interest, as no machines of this class have been previously built except for experimental purposes. They are each of 500-horse power, and are continuous current machines with four rings attached to four bars of the commutator. The alternating current is supplied by brushes rubbing on these rings, and it drives the machine as a motor. The continuous current is taken from the commutator by brushes in the ordinary way. All this machinery was made for the Cataract Construction Company by the General Electric Company, which is far the largest electrical concern in the United States. The machinery was tried in September, and it seems to work admirably. Four of these machines, with eight transformers, equal to 2000-horse power, are being put down to begin with.

The next place to be supplied is the Carborundum Company, which makes a substitute for emery, much harder, being composed of carbon and silicon raised to a high temperature in an electric furnace. They begin with 1000-horse power, and their factory is making good progress.

After that the Buffalo transmission will go on, but the selling of power in the neighbourhood is more profitable than at a distance; and many of the manufacturers, who have been holding back for two years to see how the tariff was to be settled, will now start factories, and some of them will settle at Niagara Falls to get the cheap power.

The transformer house, for raising the electric pressure from 2000 to 10,000 or 20,000 volts, is on the side of the canal opposite the power house, and these are connected by a massive stone bridge, with a covered way for carrying the cables. The concrete subway starts from the transformer house, and is at present to be used for supplying the first customers on the Company's lands.

Everything looks most promising at present, and as to the electrical works, everything indicates that any other general scheme than the one adopted would have been vastly inferior. Especially is low frequency proving itself invaluable. The continuous current could not have been got for the aluminium works without it, the motors will be far more satisfactory, and the safety and economy of the line is far higher. It was once objected that transformers could not be cheap or efficient at the low frequency proposed. Prof. Forbes held, however, that large sizes could be got even with low frequency at half the cost and at higher efficiency than anything that had been done on a small scale. This statement was based upon his own designs; and now it is entirely supported by all the manufacturers who have made designs for the work.

THE NILE RESERVOIR.

AN official memorandum upon the proposed modifications in the Assuan dam project has been drawn up by Mr. W. E. Garstin, C.M.G., Under-Secretary of State in the Egyptian Ministry of Public Works, and is published in Tuesday's *Times*. It will be remembered that an account of the schemes for the irrigation of Egypt was given in these columns a few months ago (vol. l. p. 80).

Several arguments have been brought against the Assuan cataract as the site for the dam. The first is that this site is not the only possible one to be found north of Wadi Halfa. The second, and, at first sight, the strongest, argument against the proposal is that it is impossible to lay down as an axiom that the Assuan cataract site is the only feasible one for a dam, while the river valley south of Wadi Halfa has been unexplored and unsurveyed.

Mr. Garstin criticises the arguments, and shows that the project proposed best meets the case. In this opinion he is supported by the English and Italian members of the Technical Commission—Sir Benjamin Baker and Signor Torricelli—who reported that there is only one safe site for a dam between Cairo and Wady Halfa, namely, the Assuan cataract. Subjoined is Mr. Garstin's description of the scheme. The careful consideration which has been given to the matter reflects great credit upon the Egyptian Government. Science is to be congratulated upon the action that has been taken; for the benefits that will accrue to it from the investigations which it is proposed to carry out over the whole of Nubia will be of the highest importance.

The Council of Ministers on June 3, 1894, approved in principle of the proposed dam and reservoir at the Assuan cataract, and directed the Ministry of Finance, when preparing the Budget for 1895, to occupy itself with the question of obtaining the funds necessary for the execution of this work.

The project, as then submitted to the Government, consisted of a dam with its crest at R.L. 114·00, which height would have enabled water to be stored in sufficient quantity for the requirements of Middle and Lower Egypt; in other words, for the whole country lying to the north of Assyut.

Most unfortunately the construction of this dam would have necessitated the submersion for some six months every year of the celebrated Philæ temples, as well as of a considerable number of Nubian monuments, which, although less known than those of Philæ, are of great importance to all those interested in the history of ancient Egypt.

The archæological societies of Europe, upon hearing of this proposal, protested against it in the strongest terms, and begged the Egyptian Government to reconsider its decision, and to endeavour to find some alternative scheme by which the country might reap the advantages to be derived from a storage reservoir, without sacrificing the interests of science and archæology.

The Ministry of Public Works, recognising that these protests were founded upon reasons so strong as to command respect, reconsidered the whole matter in detail, and endeavoured to find such modifications of the original scheme as might reconcile the interests of Egypt and of science.

The result of its studies is the modified project which has now been submitted to the Egyptian Government.

* * * * *

The modified scheme as at present submitted is of the nature of a compromise; it is hoped that it will satisfy the scientific world, while, at the same time, it will further the interests of this country.

It is now proposed to build a dam at Assuan with its crest at R.L. 106·00, or eight metres (26 ft.) lower than that of the original project. This work will of necessity store very much less water than the high-level dam would have done. At the same time a reservoir of this height will supply sufficient water for the wants of either Middle or Lower Egypt separately, although not for their combined areas.

This will mean that the reclamation of the country will proceed more slowly than was at first proposed; and when in course of time the country to the south has been explored a second dam can be made which will store sufficient water for the needs of the rest of Egypt.

This proposal is no new one, but has been fully discussed and estimated for in Mr. Willcocks's report upon the different sites.

The great advantage to be derived from carrying out the work in the above manner is that it will only submerge portions of the Philæ island, while it will leave the rest of the Nubian monuments untouched. A reference to Mr. Somers Clarke's note upon these latter will show that their levels are all well above that of the highest water surface in the modified project as now submitted.

As regards the Philæ temples, the main buildings will be above high-water level altogether. It is true that the South Quay wall, and some of the smaller temples, would be submerged if left unprotected. It will, however, be possible, by the construction of a low water-tight wall, or by other means, to so arrange for their protection that no damage will be done to them by the water.

To a certain extent the artistic beauty of the group will be impaired, but in a land so full of interesting relics as is Egypt, it is unfortunately impossible to carry out any great public work without in some degree interfering with some one or other of these. The only thing to be done is to try and minimise this interference as far as is possible, and in the present case the Ministry of Public Works thinks that it has succeeded in so doing.

As regards the details of the protection works to be carried out upon the Philæ island this Ministry will consult the scientific societies upon every point, and will endeavour, as far as lies in its power, to meet their wishes in the matter.

In order to still further minimise any possible loss to science which might ensue from the construction of the reservoir, it is proposed to carry out an archæological and scientific investigation of the whole of Nubia.

The English societies very rightly impressed the necessity of this work upon the Egyptian Government. The latter, although both willing and anxious to carry it out, found it impossible to do so, owing to the necessary funds not being available. If, however, the reservoir be made this difficulty at once disappears, as the cost of the above investigation will be added to the estimate of the dam itself.

The Public Works Department has been directed to put in hand as much of the work as lies within its scope and power during the ensuing winter season. Topographical surveys will be made and plans prepared; the true bearings of the temples will be fixed and the preliminary plans of all sites completed.

This portion of the work being done, the Egyptian Government will ask the European scientific societies to depute certain of their members to come to Egypt and complete the work.

In this manner it is hoped that a record and a knowledge will be obtained of this most interesting country which will be worthy of the present age, and which should be of the greatest value to the scientific world in the future.

NOTES.

WHERE the good of science is concerned, the Goldsmiths' Company is generally among the leading benefactors. With characteristic generosity, the Company has decided to make a grant of one thousand pounds for the purpose of prosecuting research work in connection with the anti-toxin treatment of diphtheria, and in aid of the manufacture of the serum. At the request of the Company, the Laboratories' Committee of the Royal College of Physicians and Surgeons have undertaken the administration of the grant.

REUTER's correspondent at Rome reports that at a quarter past six on the morning of Tuesday, November 27, an earthquake occurred at Brescia, in Lombardy. The shock was followed by subterranean rumblings. A similar, though less violent, movement was felt at Bologna at nine minutes past six. Five minutes earlier a sharp disturbance occurred at Vienna, lasting four seconds. It was followed almost immediately by a second slighter shock of two seconds' duration. Shocks were experienced about the same time at Domodossola, Mantua, Pavia, Parma, and Bergamo, while at Rome the seismic instruments gave evidence of disturbance.

THE Société Internationale des Électriciens established a central laboratory at Paris about seven years ago. The principal object of the laboratory was the preservation of electrical standards, and to afford practical electricians an opportunity for testing their various instruments. It is evident that such a laboratory offers special advantages for the investigation of questions belonging to the science and industry of electricity. These facilities have been to some extent utilised; but in order to increase the usefulness of the institution, the Society has added to it a School of Applied Electricity. This school, which will be opened on December 3, has been constructed on a plot of land granted by the city of Paris, the funds for the building having been raised by private subscription. Purely practical instruction will be given at the school. There will be two chief courses, one dealing with the industrial applications of electricity, and the other with electrometry. It is hoped that the school will be a training ground for higher work in the Central Laboratory, to which it is attached.

A RUSSIAN ethnographic exhibition will be held next year in the Champ de Mars, Paris.

THE University of Chicago is establishing a special department of botany, with Prof. J. M. Coulter at its head.

IT is announced that the printing of the important and laborious "Index Kewensis" will probably be completed about Midsummer 1895.

THE death has recently occurred of Dr. L. Schwarz, the Director of Dorpat Observatory, and the Professor of Astronomy in the University there.

THE resolutions passed by Convocation at Oxford, last summer, in favour of conferring degrees upon persons who have pursued a course of special study or research in the University, were submitted to a Congregation on Tuesday, and, after some discussion, the preamble was carried by a majority of sixty-nine votes.

A DALZIEL telegram from Halifax, Nova Scotia, dated November 27, says: "It is reported that the steamer *Falcon*, of the Peary Arctic Exploration Expedition, was wrecked on the Virgin rocks, some distance off the southern coast of Greenland in October, and that all on board perished."

THE Royal Botanical Society of Belgium has established a Committee of Vegetable Pathology, holding its sittings in the Botanic Garden at Brussels, for the purpose of affording information to nurserymen, horticulturists, and arboriculturists, respecting the diseases which attack plants, and the best mode of combating them.

IN January last, attention was drawn to the fact that a sale of seven lions had taken place at the gardens of the Royal Zoological Society of Ireland, of which number six were born in Ireland. A further batch of five cubs, all males, has recently been disposed of by the Council, after a protracted correspondence. The total exchange value for these twelve lions exceeds, we understand, £500.

AGRICULTURAL Associations seem to be waking up to the necessity for the scientific investigation of diseases and pests affecting crops. One of the resolutions passed at a largely-attended conference of agricultural societies of New Zealand held during the past summer, was—"That the services of a first-class entomologist be obtained by Government, who shall give his whole time to an examination of insect pests, with a view to their destruction." It was also resolved to request the Government to get expert opinions as to the best method of exterminating the grub in corn and grass crops.

THE promotion of an intellectual observance of Sunday is the object of the Sunday Society. At present the Society numbers among its chief aims the opening of the Natural History Museum and the South Kensington Museum on Sundays, so that the scientific collections of the nation shall enlighten a wider public. In furtherance of the general principle involved, a number of special sermons will be delivered next Sunday, this being the Society's third annual Museum Sunday; and several science and art collections will be open to members of the Society.

NEAR Dunkeld Cathedral, in the Duke of Athole's grounds, are two of the original five larch trees said to be the first introduced into this country. They were planted in 1738, and in the year 1888 they were measured with the following results:

Height	102 ft. 4 in.
Circumference:	
3 ft. from ground ...	17 ft. 2 in.
5 ft. ,, ,, ...	15 ft. 1 in.
17 ft. ,, ,, ...	12 ft. $10\frac{1}{2}$ in.
51 ft. ,, ,, ...	8 ft. 8 in.
68 ft. ,, ,, ...	6 ft. 1 in.

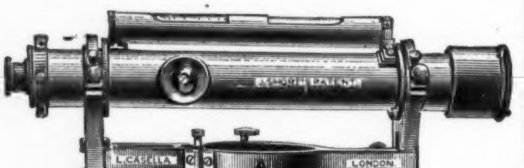
MR. WALTER B. HARRIS gave an account of his recent journey to Tafilet, at the last meeting of the Royal Geographical Society. He left Morocco city on November 1, 1893, in disguise, and crossed the Atlas at an elevation of a little over 8000 feet. In connection with this, it is worth noting that the *Bulletin* of the Paris Geographical Society (vol. xv. p. 199) contains a description, by M. Gabriel Delbrel, of a visit he paid to Tafilet last year.

DR. KARL KARSTENS, of Kiel, has made a critical revision of the various estimates of the average depth of the oceans, as arrived at by the methods of different calculators. These methods he classes as of three kinds: that of measuring areas by the planimeter on a contoured map, that of calculating the areas of successive profiles drawn at intervals of 5° of latitude apart, and that of taking the mean depth of the soundings in definite small areas, and combining these to give the mean depth of the whole. Murray and Penck had made calculations by variations of the first method, and got out the mean depth of the oceans as 3797 metres (2074 fathoms) and 3650 metres (1995 fathoms) respectively; Heiderich, by the second method, got 3438 metres (1881 fathoms); and Krümmel, by the third method, gave the figure 3320 metres (1815 fathoms). Since Krümmel's calculations were made in 1886, his student Karstens has gone over them, taking advantage of the very numerous additional soundings which have been made, and he comes to the conclusion that the average depth of the oceans as a whole is 3496 metres, or 1909 fathoms. The maximum probable value was 3632, and the minimum 3377 metres. The mean depth of the Pacific Ocean is given as 3829, of the Indian Ocean 3593, and of the Atlantic 3160 metres.

HERR P. DINSE published the first part of a discussion of the geographical characteristics of fjords in a recent number of the *Zeitschrift* of the Berlin Geographical Society (vol. xxix. p. 189). This instalment deals with the morphology of fjord-riven coasts, a subsequent paper being intended to treat of the theory of the origin of fjords, The author distinguishes between *fjords* or inlets running into steep coasts, and sometimes branching, including one or more basins deeper than the sea immediately outside, and *fjord-channels* or sounds which are similar but open at both ends, and *fjord-lakes* which are similar but closed at both ends, and separated from the sea. A farther distinction is drawn between the different kinds of inlet which superficially resemble fjords, the most contrasted in submarine configuration being the *rias* of Spain and other coasts, in which the depth of the water gradually increases from the head until it merges in the general deepening of the sea outside. The inlets of the south-west of Ireland are examples of this type, contrasting with the characteristic fjords of the

north-west of Ireland, the west of Scotland, and Norway. The greater part of the paper is occupied by an account of the distribution of the varieties of fjord-coasts in different parts of the world, and the generalisation is made that there are none of those coast-features outside the limits of lands which bear signs of recent glaciation. Herr Dinse has made a complete discussion of the principal dimensions and the exact configuration of eighty-three fjords in all parts of the world, taking his data from the largest-scale sea-charts available for each region, and these figures are printed as an appendix. The paper is illustrated by a few contoured maps of fjords, and by profiles showing their longitudinal and transverse sections.

THE Gradient-Telemeter Level, of which we give an illustration, should be of service to civil engineers and surveyors. Its novelty consists in the absence of a vertical circle, while a circle tilted out of the horizontal plane takes the place of the horizontal circle in an ordinary theodolite. So constructed, the instrument can be used to obtain linear distances, gradients, and differences in level of objects without the use of land-chain or tape. The circle is divided into natural tangents instead of degrees. When the reading is zero, the telescope is horizontal, but by rotating the circle, the telescope is inclined to the horizontal line, and the inclination is indicated by the pointer to the circle. As an example of the use of the instrument,



suppose the whole of the levelling staff to be below the horizontal line of sight. Setting the circle for a gradient of one in twenty-five, let the reading of the staff be 13·86. Now set the circle so that the index points to a gradient of one in twenty, and let the reading be 8·45. The difference between these two readings is 5·41, and, eliminating the decimal point, the number obtained—541—is the horizontal distance in feet between the instrument and the staff, without any further calculation. Other pairs of gradients may be used, and the difference of their staff-readings gives the linear horizontal distance. The instrument thus greatly simplifies many surveying operations.

A COLLECTION of valuable notes on the aborigines of various parts of Australia is given in the November *Journal* of the Anthropological Institute. The notes, which take the form of answers to questions on the manners, customs, religions, superstitions, &c., of the native tribes of Australia, have been collected by Dr. E. C. Stirling. The natives are divided into innumerable tribes, and different customs prevail every two hundred miles or so. Arithmetic is beyond their comprehension. Most of the tribes appear to have distinctive names for one and two, but for three they say two and one; for four, they say two and two; and for five, two two and one. The fingers are used in counting, but numbers beyond five are very seldom used. Numbers greater than ten are usually described as "plenty" and "many," and explained by opening and closing the hands several times. The tribes have a very limited knowledge of the measurement of time. They tell the time of day by the sun, and speak of the different times of the position of the sun. They reckon by so many moons, and determine the year by the seasons; but they have no knowledge of the constellations, nor have they any names for the months, or moons, as they call them, or any recognised beginning of the year, nor artificial timekeeper of the nature of sun-dials, though the lengths of the shadows of trees is used by some tribes to determine the time of day. The heavenly bodies are not worshipped, neither are any ceremonies performed at the new moon, sunrise, sunset, the solstices, equinoxes, &c. The Milky Way is supposed to be the largest creek or river. A fresh sun is believed to shine every day, and the phases of the moon are explained by the prevailing course of the wind and prevailing quarter of the rain. Eclipses, rain, thunder, lightning, rainbows, wind, and Aurora Australis are supposed to be the work of evil spirits. Many other beliefs are described in the notes, which will be read with great interest by every student of anthropology and folk-lore.

THE mutual alterations effected between an invading igneous mass and the rock which it invades, have long been the subject of much study and speculation. An interesting case, presenting some exceptional features, has recently been described by Prof. Cole (*Trans. Roy. Dublin Soc.* vol. v. (n.s.) No. 5). On the coast of Co. Down, the Ordovician strata are cut by numerous basic dykes. In some cases, material of more acid composition has intruded at a later date, and, forcing its way along the same lines of weakness as the earlier basic material, has produced "compound" dykes. It is one such compound dyke, at Glasdrumman, consisting of basaltic andesite rifted lengthwise by later eurite, that Prof. Cole describes in detail. The new-comer has re-melted the more easily fusible andesite, and the two magmas have mixed along the contact. Various stages of mixture are described, but the most interesting facts are those concerning the large crystals of quartz and felspar which abound in the marginal portions of the eurite. These crystals have evidently consolidated under other, and earlier, conditions than the main mass of the rock, since portions of the eurite-matrix have eaten their way into them before solidifying. But crystals undoubtedly so corroded are also found in the andesite, into which they must have floated from the eurite. The presence in a rock of crystals of evident foreign origin is no new thing, but hitherto it has, in all such cases, been either shown or assumed that they were caught up by the rock in which they are found from some other into which it had intruded; whereas, here, it is the invading eurite that has parted with its crystals for the benefit of its host. Thus the common dictum, that "an enclosed block must be older than the rock immediately enclosing it," is apparently controverted; yet, since consolidation is the datum from which age is measured, it becomes a decidedly nice point in geological nomenclature whether a partially re-fused rock can be allowed to pass as entirely older than the rock which has re-melted it.

SOME interesting particulars concerning "aventurine glass," one of the most curious products of the world-renowned glass-works at Murano, near Venice, are given in the current number of the *American Journal of Science*, by Mr. Henry S. Washington. Its name is derived from its supposed discovery "by

chance," some brass filings having been dropped accidentally into a pot of molten glass. After the late Dr. Salviati's revival of the glass industry at Murano it was rediscovered, but the present process is a trade secret. The best glass is of a copper-brown colour, and transparent to translucent in thin flakes, showing on the edges a pale brown colour. It is filled with innumerable small flakes and spangles of a slightly brownish yellow colour and brilliant metallic lustre, consisting of crystallised copper. Under the microscope the glass shows a porphyritic structure, the ground-mass being composed of a perfectly clear and colourless glass basis. The crystallised portions consist of large phenocrysts, small phenocrysts, and microlites. The former range in diameter from 0·05 to 0·12 mm., are tubular and extremely thin, the thickness scarcely exceeding 0·002 mm., and are perfectly opaque notwithstanding their excessive tenuity. Most of them are hexagonal in outline, the hexagons being of almost ideal symmetry; but equilateral triangles, which occasionally show truncated angles, also occur. The smaller phenocrysts are much more diverse in crystalline form, and may be generally grouped in one of three divisions: cubo-octahedral forms, octahedra, and twins. They occur in portions of the glass free from the larger phenocrysts, but filled with abundant microlites, from which they are usually separated by a clear zone. The copper has evidently crystallised out from solution in the molten glass exactly like a salt from water, following Lehmann's laws of crystal growth in solutions. Mr. Washington is of the opinion that the glass is produced by melting together glass, cuprous oxide, and some reducing agent, such as siderite; and that FeO is in this case the reducing agent is shown by the greenish colour of the imperfect glass, which is not the blue green of copper, but the yellow green of ferrous glass, and perhaps due to too large a quantity of reducing agent.

AT the last meeting of the French Physical Society, MM. Cailletet and Colardeau read a very interesting paper on the condensation of the gases produced by electrolysis on electrodes formed of metals of the platinum group. It is well known that when acidulated water is electrolysed by means of platinum electrodes that, on removing the battery and connecting the electrodes, a current is obtained in the opposite direction to the original current used to perform the electrolysis. This current, which only lasts for a short time, is explained by the recombination of the hydrogen and oxygen which coat the platinum electrodes. The authors, taking advantage of the well-known property of finely-divided platinum of occluding gas in large quantity, were led to use masses of finely-divided platinum contained in silk bags as electrodes, and in this way, using electrodes weighing six grms. each, a current was obtained, after disconnecting the battery, which continued for some time, and was of sufficient strength to ring an electric bell. By enclosing this form of the cell in a receiver, and compressing the air within the receiver, the following results were obtained:—With an additional pressure of one atmosphere, the E.M.F. immediately after charging was 1·8 volts, which fell regularly to zero when the cell was discharged. On increasing the pressure the character of the discharge-curve obtained entirely alters, and consists of three parts. (1) A portion in which the intensity of the discharge current rapidly diminishes. (2) A portion during which the current remains constant. This period occupies the major part of the time occupied in the discharge, and the E.M.F. at this time is about one volt. (3) A second period in which the current diminishes and finally becomes zero. The capacity of such an accumulator, the weight of the two electrodes being 1 kilogram, is, under a pressure of 580 atmospheres, 56 ampere-hours, while a current of 100 amperes can be obtained. To obtain the best possible result, the negative electrode should contain three times the weight of platinum in the positive electrode. With finely-divided palladium a storage capacity of 176 ampere-hours per kilogram of palladium was obtained at a pressure of 600 atmospheres. It is interesting to note that the storage capacity of an ordinary lead accumulator is about 15 ampere-hours per kilo of metal.

LEON GUIGNARD, in the *Journal de Botanique*, adds another important contribution to our knowledge of the centrospheres of plant-nuclei, entitled "Sur l'origine des sphères directrices." It will be remembered that this author was the first to demonstrate the existence of these structures in vegetable cells (*Comptes-rendus*, 1891), and also, in a later paper, to describe their behaviour during the origin of the sexual cells and the part they take in the phenomena of fertilisation (*Ann. des Sc. Nat.* 1891). In these earlier works Guignard already figured many resting nuclei with the centrospheres lying outside the nuclear membrane, and usually in close proximity to it, while within the nuclear membrane are to be seen one or more nucleoli. Strasburger also observed and figured centrospheres outside resting nuclei in *Sphacelaria scoparia* (*Hist. Beiträge*, iv. p. 52). Recently, however, G. Karsten was led to believe, from a study of the relations of the nucleoli and centrosomes in the mother cells of the spores of *Psilotum triquetrum* that the centrosomes (or minute bodies included in the centrospheres) owe their origin to the nucleoli, and after karyokinesis are re-included as nucleoli in the daughter nuclei. Guignard's last paper is chiefly concerned with an examination of this point, and he comes to the conclusion that prior to karyokinesis the centrospheres in the cells of the sporangia of *P. triquetrum* are external to the nuclear membrane, and that after karyokinesis, while some of the small nucleoli which have not disappeared during the division of the nucleus are re-included within the nuclear membranes of the daughter nuclei, they remain external to them.

A RECENT number of the *Minnesota Botanical Studies* contains a bibliography on the subject of the fixation of free nitrogen by plants, embracing over 600 titles.

THE report of the fifth meeting of the Australasian Association for the Advancement of Science, held at Adelaide, in September 1893, has just reached us from Sydney, where the permanent office of the Association is situated.

MESSRS. RIVINGTON, PERCIVAL, AND CO. have published a third edition of "Practical Inorganic Chemistry," by Mr E. J. Cox. The book is intended for students preparing for the elementary practical chemistry examination of the Department of Science and Art.

IN the form of "Bulletin No. 56," Mr. P. H. Mell, the State Botanist for Alabama, records the result of a series of observations on the crossing of different varieties of the cotton-plant at the Agricultural Experiment Station at Auburn. The plant is pollinated by the agency of the wind and of insects, and he finds inter-crossing to have a material effect in increasing the strength of the fibre.

MISS ORMEROD will issue in a few days an abstract of information on the history and habits of that seriously destructive cattle-pest, the Warble Fly or Ox Bot Fly. The description will be very fully illustrated, and will be an epitome of the knowledge and experience gained up to the present time, and especially during the years 1884 to 1894. It will deal practically with means of prevention and remedy. The publishers are Messrs. Simpkin, Marshall, Hamilton, Kent, and Co.

AN "Artificial Spectrum Top," devised by Mr. C. E. Benham, and sold by Messrs. Newton and Co., furnishes an interesting phenomenon to students of physiological optics. The top consists of a disc, one half of which is black, while the

other half has twelve arcs of concentric circles drawn upon it. Each arc subtends an angle of forty-five degrees. In the first quadrant there are three such concentric arcs, in the next three more, and so on; the only difference being that the arcs are parts of circles of which the radii increase in arithmetical progression. Each quadrant thus contains a group of arcs differing in length from those of the other quadrants. The curious point is that when this disc is revolved, the impression of concentric circles of different colours is produced upon the retina. If the direction of rotation is reversed, the order of these tints is also reversed. The cause of these appearances does not appear to have been exactly worked out.

THE additions to the Zoological Society's Gardens during the past week include a Black Lemur (*Lemur macaco*, ♂) from Madagascar, presented by Mr. Roche; a Snowy Owl (*Nyctea scandiaca*), captured in mid-Atlantic, 700 miles from land, presented by Mr. Harston Eagle: two Levaillant's Cynictis (*Cynictis levaillanti*), two Domestic Sheep (*Ovis aries*, var.), two Puff Adders (*Vipera arietans*), a Cape Bucephalus (*Bucephalus capensis*), six Hispid Lizards (*Agama hispida*), five Rough-scaled Lizards (*Zonurus corydlus*), a Delalande's Lizard (*Nucras delalandi*), a Crossed Snake (*Psammophis crucifer*) from South Africa, two Bennett's Tree Kangaroos (*Dendrolagus bennethanus*) from North Queensland; an Allied Goshawk (*Astur approximans*), three Long-necked Chelodines (*Chelodina longicollis*), twenty-two Golden Tree Frogs (*Hyla aurea*), seventeen White's Tree Frogs (*Hyla cærula*) from Australia, a Spix's Macaw (*Ara spixi*) from North Brazil, deposited; two Caroline Conures (*Conurus carolinensis*) from North America, purchased; two Queensland Tree Kangaroos (*Dendrolagus lumholtzi*, ♂ ♀) from Queensland; four Brush Turkeys (*Talegalla lathami*, 4 ♂) from Australia, received in exchange.

OUR ASTRONOMICAL COLUMN.

THE PARALLAX OF NEBULA *h* 2241.—At the time when Dr. Wilsing took photographs of the nebula B.D. + 41°·4004 for the determination of parallax, he obtained also a series of negatives of B.D. + 41°·4773 (*h* 2241) for the same purpose. This nebula is almost ring-shaped, and displays a central condensation. The latter appears more distinct on the photographic plates than can be seen by eye observations, and its contour is only sufficiently sharp for micrometric measurements on the best plates, so that the centre of the whole apparent disc has been generally used. From June 1892 to August of the following year, 33 plates were obtained, 31 of which have been used in this research. Six comparison stars, the positions of which were taken from the Bonn zones, have been adopted. In the account of the result obtained (*Astro. Nach.* No. 3261), Dr. Wilsing gives a table showing the deduced distances of the nebula from the two comparison stars 3 and 6. A second table contains the mean monthly values of these distances with their differences from the whole *mean* value obtained from all the measurements, together with the most probable errors of the measurements.

The following table shows these differences between the total and monthly means for the two stars 3 and 6:

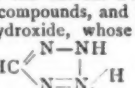
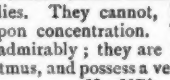
	[N, 3]	[N, 6]	Prob. error.	No. of plates.
	″	″	″	
1892 June 21	−0·03	+0·28	±0·08	5
July 13	+0·11	+0·03	0·06	9
Aug. 9	+0·07	−0·04	0·13	2
Sept. 25	+0·01	−0·06	0·08	5
Oct. 4	−0·13	+0·01	0·13	2
Nov. 8	+0·20	+0·03	0·10	3
Dec. 22	−0·53	−0·44	0·18	1
1893 Feb. 4	−0·13	−0·34	0·18	1
July	−0·08	−0·19	0·13	2
Aug.	−0·53	−0·14	0·18	1

These differences, when considered in relation with the probable errors of the measurements, have as Dr. Wilsing suggests, to be cautiously dealt with, and he is led to conclude from this series of measures that the relative parallax of this nebula does not exceed one or two tenths of a second of arc.

A POSSIBLE NEW ZONE OF ASTEROIDS.—The secular variations of the orbits of the four inner planets has lately occupied Prof. Newcomb's attention, with the result that several elements have been found to vary in a manner unaccounted for by existing theory. (*Astronomical Journal*, No. 327.) "These anomalies," says Prof. Newcomb, "cannot be simultaneously explained either by an intra-mercurial zone of planets, by the action of matter reflecting the zodiacal light, or by a deviation of gravitation from the usually accepted law. The uncertainty as to the mass of Mercury makes the construction even of a working hypothesis difficult; but apart from all considerations of probabilities, *à priori*, the hypothesis which best represents observations, is that of a ring of planetoids of small eccentricity a little outside the orbit of Mercury, and a little more inclined to the ecliptic. The total mass of the ring may range from one-fiftieth to, perhaps, one three-hundredth of the mass of Venus, according to its distance from Mercury." Prof. Newcomb intends to carefully investigate the matter in order "to decide whether the results of the hypothesis are such as to counterbalance its extreme improbability."

A NEW COMET.—*Edinburgh Circular*, No. 43, dated November 23, states that a telegram received from the Central Astronomical Station at Kiel announces the discovery of a very faint comet, by Mr. Edward Swift, at 8 p.m., Californian time, on the 20th inst. It was situated in Right Ascension, 22h. 18m. 24s., and South Declination, 13° 7′, and was moving slowly towards the east.

A NEW SERIES OF NITROGEN COMPOUNDS.

ANOTHER new series of nitrogen compounds, containing four atoms of that element along with one atom of carbon in a closed chain, are described by Prof. v. Pechmann and Herr Runge in the current *Berichte*. They are termed "tetrazolium" compounds, and the parent base of the series is tetrazolium hydroxide, whose constitution is represented by the formula . The fundamental base itself has not yet been isolated; the compounds prepared comprise the derivative in which the two hydrogen atoms directly attached to the two end nitrogen atoms are replaced by phenyl, together with a large number of salts of this base, formed by replacement of the hydroxyl by halogens or other acid radicles just as in the case of metallic hydroxides. The hydrogen atom attached to the carbon is likewise capable of replacement by many organic radicles, so that a large number of still more complicated bases have likewise been prepared, together with their corresponding salts. The hydroxides of this new series are characterised by possessing strong basic properties. They may all be prepared most conveniently from their chlorides, by the action upon them of silver oxide. They are extremely soluble in water, but are completely precipitated from their solutions by ether. The aqueous solutions absorb carbon dioxide and behave very much like caustic alkalies. They cannot, however, be crystallised, forming resins upon concentration. The salts, on the other hand, crystallise admirably; they are usually soluble in water, react neutral to litmus, and possess a very bitter taste. Diphenyl tetrazolium chloride, , which may be taken as a typical salt of the series, crystallises in colourless radiating groups of needles very sensitive to light, which renders them yellow. The aqueous solution yields a flesh-coloured precipitate of a chloroplatinate with platinum chloride, and the double salt may be crystallised from hot water. A crystalline double chloride is likewise produced with gold chloride. The addition of a soluble nitrate or iodide causes the precipitation of the difficultly soluble nitrate or iodide of the base. A solution of iodine in potassium iodide precipitates an iodine addition product, which can be crystallised from alcohol in beautiful brown tabular crystals exhibiting a violet reflection. The parent base is produced in solution upon the addition of silver oxide, silver

chloride being likewise formed. The chloride is reduced by ammonium sulphide to a compound of the constitution:

$$HC\begin{matrix} \diagup N-NHPh \\ \diagdown N-NPh \end{matrix}$$

, a substance which Prof. v. Pechmann has previously described, and which is interesting as forming the starting-point for the preparation of the new series. For the chloride may at once be prepared from this latter substance by oxidation with amyl nitrite and hydrochloric acid. The substance is readily prepared by the action of diazobenzene chloride upon malonic acid, constituting the insoluble product of the reaction. It is of considerable interest to observe that the main product of the dry distillation of diphenyl tetrazolium chloride is azobenzene.

THE SCIENTIFIC INVESTIGATIONS OF THE SCOTTISH FISHERY BOARD.

THE Twelth Annual Report of the Fishery Board for Scotland (Part III. Scientific Investigations for 1893) contains a quantity of new information upon fishery problems, and marks an important stage in the history of the Board, owing to the successful inauguration during the past year of a hatchery at Dunbar for the artificial propagation of marine food-fishes.

A number of important conclusions are formulated by the Board upon various matters. In the first place, the closure of the territorial waters to beam-trawling is admitted to have had no appreciable effect towards arresting the continued decline in the supply of flat-fishes, although the interdicted area has been very large. The greater part of the territorial waters of the East Coast, the Firth of Forth, and St. Andrews Bay have been protected (except for experimental purposes) since 1886, and this area was greatly extended in 1889, when practically the whole of the territorial waters and several extensive bays (the Firth of Clyde and the Moray Firth) were closed against the operations of the beam-trawler. The reason for the failure of this method of protection is sought for in the fact that the present protected area does not embrace the spawning grounds of food-fishes, except in the case of the Moray Firth. It is most unfortunate that, from lack of a sufficiently seaworthy vessel, the Board has been unable to devote the same attention to the Moray Firth as to the Firth of Forth and St. Andrews Bay, for statistics upon the condition of the Moray Firth throughout the year would have been invaluable. But it can be definitely asserted that the mere protection of areas that do not include spawning grounds is practically useless to prevent depletion of the home fisheries. The recommendation of the recent Parliamentary Committee that the present territorial limit should be considerably extended, is accordingly endorsed; and, in order to ensure the enclosure of the more important breeding-grounds, the Board emphasises its recommendation of the previous year, that the limit of jurisdiction should be extended to ten or twelve miles from shore.

Experiments have been made upon the effects of alteration in the size of the mesh of the beam-trawl upon the capture of immature fish. It was found that, contrary to the opinion of most practical men at the recent Parliamentary inquiry, the size of the mesh has a most appreciable influence in determining the size of the fish captured. Dr. Fulton's effective experimental trawlings show that the proportion of fishes that escape through the cod-end of the trawl increases greatly as the width of the meshes is enlarged.

Prof. M'Intosh gives an interesting review of the trawling question in general, and includes a valuable sketch of the changes which have taken place during the past ten years in trawling-vessels and their apparatus. Reasons are adduced which tend to show that line-fishing is quite as destructive as trawling to immature round fishes, such as cod and haddock; and it is maintained that the perennial abundance of the floating fauna, of which larval stages of bottom-animals form so large a proportion, is sufficient to prevent the trawling-grounds from being depleted of fish-food to any serious extent.

The volume includes a number of papers of a more purely biological character upon the development of fishes, on the invertebrate fauna of the Firth of Forth and certain inland lochs, on the oviposition and rate of growth of the sand-eel and certain other fishes, and on some seasonal changes in the histology of fishes. Two papers on the osteology of the tunny and on the anatomy of the pectoral arch in the gurnard seem to us to be completely out of place in an official publication ostensibly devoted to fishery investigations, with which they have nothing to do.

Turning to Prof. M'Intosh's "Remarks on Trawling," justifiable as his general position appears to be, he has, nevertheless, left himself open to criticism on a number of minor points. It is difficult to reconcile with the statistics of the *Garland* trawlings the Professor's remark that "the closure of the inshore waters—*e.g.* St. Andrews Bay—must have conduced to the prosperity of the turbot and the brill of that neighbourhood, most of the turbot (ranging from 9 to 11 inches) *which formerly were captured by the trawlers* now being unmolested" (p. 167). For in Dr. Fulton's introductory report on the work of the *Garland* it is stated that in St. Andrews Bay, as in the Firth of Forth, there was an actual decrease in 1893 of "turbot and brill" in the closed areas as compared with 1892 (p. 26); and the decrease of flat-fishes in general during the eight years of closure is demonstrated on p. 33. Moreover, out of the twenty-four experimental trawlings conducted by the *Garland* in the closed waters of St. Andrews Bay in 1893 only two turbot, and no brill at all, were obtained. Indeed, the average take of turbot was twice as great in the unprotected as in the protected areas of the Bay (p. 42).

In one of the most interesting sections of his "Remarks" (p. 184), Prof. M'Intosh discusses the "effect of trawling on the invertebrate fauna of the sea-bottom (forming fish-food)." It is full of valuable observations from the rich stores of the Professor's experience, but, as an argument, seems to us to be vitiated by a very questionable assumption which underlies it, viz. that all invertebrate life on the sea-bottom furnishes food for fishes. Half the groups, at least, which are mentioned by the Professor in this connection should, in our opinion, be eliminated, viz. sponges, hydroids, anemones, alcyonaria, star-fishes, balani, and ascidians, although we are quite prepared to allow that now and then, in exceptional cases, particular species of some of these groups may be swallowed by fishes. Therefore the Professor's argument that the trawl causes little impoverishment of the supply of fish-food, owing to the rapid powers of growth and repair which the above groups (among some others) possess, is seriously impaired.

In Mr. Harald Dannevig's Report (p. 211) we notice the interesting observation that fishes which spawn during the night in open ponds will do so during the day also if the pond be darkened.

Coming to the biological investigations, we observe that Prof. M'Intosh has overlooked (p. 227) the fact that the Norwegian Topknot (*Zeugopterus norvegicus*) has been recorded by Mr. Cunningham as occurring in considerable numbers at Plymouth (*Jour. M.B.A.* ii. 1892, p. 325). In connection with Mr. Sandeman's investigations on seasonal changes in the histology of fishes, attention may be drawn to another paper by Mr. Cunningham (*Jour. M.B.A.* ii. 1891, pp. 16-42), in which a number of remarkable histological changes are shown to take place in the female conger during the period of the maturation of her eggs.

The main results of Mr. Dickson's physical investigations in the Faröe-Shetland seas seem to us to be of profound importance. If, as he contends, a mass of Atlantic water is every year admitted through the Faröe-Shetland Channel, winds round the Shetlands, and bores its way down the eastern coasts of Scotland in the summer months, guided by a bank of dense water in the upper regions of the North Sea, it is clear that we have at once an explanation of numbers of isolated facts of occasional or periodic distribution of pelagic animals in those regions, which have hitherto seemed merely freaks of Neptune or Æolus. And it cannot be doubted that a further extension of such investigations as Mr. Dickson has been carrying out in H.M.S. *Jackal*, if coupled with a corresponding survey of the pelagic fauna, will provide the long-sought solution of the migrations of the herring and other nomad fishes round our coasts.

In congratulating the Board upon its scientific achievements for the year, we cannot help expressing our intense regret that the recently vacant chairmanship was not offered by the Government to Dr. John Murray. His experience and energy would at all times be invaluable, but at the present juncture, when so many important fishery problems of a physico-biological nature are pressing for solution, the loss to the Board and to the country of his counsel and aid is incalculable.

W. G.

SIR JOHN DONNELLY ON TECHNICAL EDUCATION.

AT the first ordinary meeting of the new session of the Society of Arts, Major-General Sir John Donnelly delivered an address in which he dealt with some points in the history of the Society, and especially with those connected with the promotion of education. The following is a condensed report of his remarks bearing upon the development of technical instruction:—

In 1868, a Conference on Technical Education was held by the Society of Arts, and shortly afterwards—on March 24, 1868—on the motion of Mr., now Sir B. Samuelson, Bart., the House of Commons granted a Select Committee, of which he was appointed chairman, "to inquire into the provisions for giving instruction in theoretical and applied science to the industrial classes." The first three of their conclusions were—(1) That, with the view to enable the working classes to benefit by scientific instruction, it is of the utmost importance that efficient elementary instruction should be within the reach of every child; (2) that unless regular attendance of the children for a sufficient period can be obtained, little can be done in the way of their scientific instruction; (3) that elementary instruction in drawing, in physical geography, and in the phenomena of nature should be given in elementary schools. Throughout these discussions the object-lesson afforded by the Paris Exhibition of 1867 was universally acknowledged to be the main feature of the movement.

Sir John Donnelly brought before the Society in 1872 a scheme for examinations in technology, which were to be supplementary to the examinations of the Science and Art Department. The scheme did not meet with much enthusiasm, and manufacturers set themselves against it on the grounds that trade secrets should not be the talk of the class-room. However, since then the examinations have been very largely developed by the City and Guilds of London Institute.

Owing to a set of circumstances, with which everyone is now thoroughly conversant, there was, shortly after the passing of the Technical Instruction Act, in 1889, a great windfall for technical instruction. Under the Customs and Excise Act of 1890, the residue, amounting to something over three-quarters of a million of money in England and Wales, became applicable to technical education. It has been so applied very largely. From a recent return it appears that, of the forty-nine County Councils, excluding Wales and Monmouth, forty-one are applying the whole, and eight a part of the residue to technical education. Of the sixty-one County Boroughs, fifty-three are applying the whole, and seven a part of the residue to technical education; while in one case only (the County Borough of Preston) the residue is being applied wholly to relief of rates. Further than this, ten County Boroughs are, in addition, levying a rate under the Technical Instruction Acts.

For the year 1893–94, the forty-nine County Councils have allocated about £465,000, and the County Boroughs about £161,000 from the Customs and Excise grant, besides raising over £12,700 by rates. This makes a total of almost exactly £626,000 provided in England alone for technical instruction for the year, independent of the grants from the Science and Art Department.

It is purely at the option of local authorities whether they apply the "beer" money to technical education, or whether they use it in relief of the rates. It is very gratifying to see the extent to which they have devoted it to the former object, and it shows that the operations of the Science and Art Department, the Society of Arts, the City and Guilds of London, and other bodies which had previously been engaged in the movement, have not been unfruitful. But unquestionably a great danger lurks around a sudden outburst or zeal of this kind. How far have the public generally been convinced of the efficacy of science and art and technical instruction, and the advantage of spending all the money on it, rather than in relief of rates? or how far have they been only momentarily carried away unwilling captives at the chariot-wheels of the enthusiasts? How soon will the pendulum of public opinion which has been so suddenly and so severely forced in one direction swing back again? Or—a still greater danger—how soon will the critic, the cynic, and the "practical" man commence their innings by asking to have the account balanced and the profit shown? There are already murmurings in the air: did not our forefathers get on very well without technical education? or how is it that we stand—or, at least, stood—at the head of manufacturing and commercial fame and engineering ability? At all events, if you cannot show any fruit let us have an inquiry; dig up the plant and have a look at its roots to see that we have planted the right sort.

Now what is this "technical instruction" with which the country is so much occupied at the present time? It is defined in the Act of 1889 as instruction in the principles of science and art applicable to industries, and in the application of special branches of science and art to specific industries or employments, as well as in modern languages and commercial and agricultural subjects, but not in teaching the practice of any trade, or industry, or employment.

The Act, in fact, provides for instruction in technology and not in technics. Besides, though the definition clause is careful to indicate that the principles of science and art are to be cultivated, the title of the Act appeals to the sympathy of the great mass who always clamour for a short cut—some way for arriving at the money-making application of science and of art without that preliminary study which is so laborious and apparently unremunerative.

After dwelling upon changes of style in artistic work and design, Sir John Donnelly went on to say that every now and then we hear a great outcry against South Kensington and its "system." And if South Kensington now, why not in a few years hence the technical schools and courses of instruction which are being set up with so much care and thought in all parts of the country? This danger is already felt by many who are interested in technical instruction. The Science and Art Department could always point to the fact that, if its science teaching was wrong, it erred in good company, for the syllabuses were prepared, and the examinations were conducted by some of the most eminent men of science of the day.

But to whom can the local authorities under the Technical Instruction Act appeal? It seemed to him that for their own satisfaction, and for the future stability of technical instruction, they will desire, instead of remaining, as it were, isolated and self-contained, to have an influential examining and inspecting board, to which they might refer, if they found it desirable, for assistance and advice. There are at present several bodies partially covering the ground—but only partially, and there is the great disadvantage of a want of unity. He threw out the suggestion that the Society of Arts, which is at present covering part of the field, should take the initiative in bringing all these bodies together, so that they may form some kind of joint board, or at least co-operate.

THE BATTLE OF THE FORESTS.[1]

I.

THE earth is a potential forest. Given time, freedom from geologic revolutions and from interference by man, the tree growth must finally dominate everywhere, with few excepted localities.

Its perennial nature and its elevation in height above all other forms of vegetation, together with its remarkable recuperative powers, assure to the arborescent flora this final victory over its competitors.

So impressed was Dr. Asa Gray with the persistence of individual tree life that he questioned whether a tree need ever die: "For the tree (unlike the animal) is gradually developed by the successive addition of new parts. It annually renews not only its buds and leaves, but its wood and its roots; everything, indeed, that is concerned in its life and growth. Thus, like the fabled Æson, being restored from the decrepitude of age to the bloom of early youth, the most recent branchlets being placed by means of the latest layer of wood in favourable communication with the newly-formed roots, and these extending at a corresponding rate into fresh soil, why has not the tree all the conditions of existence in the thousandth that is possessed of in the hundredth or the tenth year of its age?

"The old and central part of the trunk may, indeed, decay, but this is of little moment, so long as new layers are regularly formed at the circumference. The tree survives, and it is difficult to show that it is liable to death from old age in any proper sense of the term."

[1] A lecture delivered by Prof. B. E. Fernow, Chief of the Forestry Department of Agriculture, U.S.A., during the Brooklyn meeting of the American Association for the Advancement of Science.

However this may be, we know trees succumb to external causes. Nevertheless, they are perennial enough to outlive aught else, "to be the oldest inhabitants of the globe, to be more ancient than any human monument, and exhibit in some of the survivors a living antiquity compared with which the mouldering relics of the earliest Egyptian civilisation, the pyramids themselves, are but structures of yesterday." The dragon trees, so called, found on the island of Teneriffe, off the African coast, are believed to be many thousand years old. The largest is only 15 feet in diameter and 75 feet high. Our sequoias are more rapid growers, and attain in 3000 to 4000 years, which may be the highest age of living ones, more than double these dimensions.

While this persistence of life is one of the attributes which in the battle for life must count of immeasurable advantage, the other characteristic of arboreal development, its elevation in height above everything living, is no less an advantage over all competitors for light, the source of all life. Can there be any doubt that in this competition size must ultimately triumph, and the undersized go to the wall?

Endowed with these weapons of defensive and offensive warfare, forest growth, through all geologic ages during which the earth supported life, has endeavoured, and no doubt to a degree succeeded, in gaining possession of the earth's surface.

As *terra firma* increased emerging in islands above the ocean, so increased the area of forest, changing in composition to correspond with the change of physical and climatic conditions.

As early as the Devonian age, when but a small part of our continent was formed, the mud flats and sand reefs, ever increasing by new accumulations under the action of the waves and currents of the ocean, were changed from a bare and lifeless world above tide level to one of forest-clad hills and dales.

Not only were such quaint forms as the tree rushes Calamites, Lepidodendron and Sigillaria present, but the prototype of our pine, the Dadoxylon, had made its appearance.

The same class of flowerless plants known as vascular cryptogams, with the colossal tree ferns added, became more numerous and luxuriant in the Carboniferous age, as well as the towering Sigillaria and coniferous Dadoxylon. This vegetation probably spread over all the dry land, but the thick deposits of vegetable remains accumulating in the marshy places under dense jungle growth and in shallow lakes with floating islands, were finally, in the course of geologic revolutions, turned into the great coal fields.

In those and subsequent geologic times some of the floral types vanished altogether and new ones originated, so that at the end of Mesozoic times a considerable change in the landscape had taken place.

In addition to coniferous trees, the palms appeared, and also the first of angiosperms, such as the oak, dogwood, beech, poplar, willow, sassafras, and tulip tree. Species increased in numbers, adapted to all sorts of conditions; the forest in a most varied and luxuriant form climbed the mountain-sides to the very crests, and covered the land to the very poles with a flora of tropical and semi-tropical species.

Then came the levelling process and other changes of post-Tertiary or Quaternary times; the glaciation of lands in northern latitudes, with the consequent changes of climate, which brought about corresponding changes in the ranks of the forest, killing out many of the species around the north pole. Only the hardier races survived, and these were driven southward in a veritable rout.

When these boreal times subsided in a degree, the advance of the forest was as sure as before, but the battle order was somewhat changed to suit the new conditions of soil and climate. Only the hardiest tribes could regain the northernmost posts, and these found their former places of occupancy changed by fluvial and lacustrine formations and the drifts borne and deposited by the ice-sheets, while some by their constitution were entirely unfitted from engaging in a northern campaign, or found insurmountable barriers in the refrigerated east-west elevations of Europe and Western Asia.

In addition, there had come new troubles from volcanic eruptions, which continually wrested the reconquered ground from the persistent advance guards of the arboreal army, annihilating them again and again.

Finally, when the more settled geologic and climatic conditions of the present era arrived, and the sun rose over a world ready for human habitation, man found what we are pleased to call the virgin forest—a product of long-continued evolutionary changes—occupying most, if not all the dry land, and ever intent upon extending its realm.

This prehistoric view of the battle of the forest cannot be left without giving some historic evidences of its truth.

Not only have palæobotanists unearthed the remnants of the circumpolar flora, which give evidence that it resembled that of present tropic and semi-tropic composition, but they have also shown that sequoias, magnolias, liquidambars and hickories existed in Europe and on our own continent in regions where they are now extinct. We have also evidences of the repeated successes and reverses of the forest in its attempts to establish itself through long geologic transformations.

One of the most interesting evidences of these vicissitudes in the battle of the forest is represented in a section of Amethyst Mountain in Yellowstone National Park, exhibiting the remains of fifteen forest growths, one above the other, buried in the lava. Again and again the forest subdued the inhospitable excoriations; again and again it had to yield to superior force.

Among these petrified witnesses of former forest glory, magnolia, oak, tulip tree, sassasfras, linden and ash have been identified, accompanying the sequoia in regions where now only the hardiest conifer growths of pines and spruces find a congenial climate.

As the forest formed and spread thus during the course of ages, so does it form and spread to-day, unless man, driven by the increasing needs of existence, checks its progress and reduces its area by the cultivation of the soil. This natural extension of the forest cover or afforestation takes place readily whenever soil and climate is favourable, but it is accomplished just as surely, though infinitely slower, in unfavourable situations. On the naked rock, the coarse detritus and gravel beds, on the purely siliceous sand deposits of river and ocean, or in the hot dry plains, the preliminary pioneer work of the lower vegetation is required. Algæ, lichens, mosses, grasses, herbs, and shrubs must precede to cultivate the naked rock, to mellow the rough moisture by shading the ground, and gradually render it fit for the abode of the forest monarch. The army of soil-makers and soil-breakers, the pioneers, as it were, of the forest, are a hardy race, making less demands for their support than those that follow. They come from different tribes, according to the soil conditions in which they have to battle.

The aspen (*Populus tremuloides*) is one of these forerunners, which is readily wafted by the winds over hundreds of miles, readily germinates and rapidly grows under exposure to full sunlight, and even now in the Rocky Mountains and elsewhere quickly takes possession of the areas which man has ruthlessly destroyed by fire. This humble and ubiquitous, but otherwise almost useless, tree is nature's restorative, covering the sores and scalds of the burnt mountain side, the balm poured upon grievous wounds. Though short-lived, with its light summer foliage turning into brilliant golden autumn hues, it gives grateful shade and preserves from the thirsty sun and wind some moisture, so that the better kinds may thrive and take its place when it has fulfilled its mission.

One of the shrubs or half-trees which first take possession of the soil in the western mountain country is the so-called mountain mahogany (*Cercocarpus ledifolius*), covering the bared slopes after the fire has killed the old timber.

In other regions, as on the prairies of Iowa and Illinois, hazel bushes, or in the mountains of Pennsylvania and the Alleghanies in general, ericaceous shrubs like the laurel and rhododendrons or hawthorn, viburnum and wild cherry are the first comers, while along water-courses alders and willows crowd even the water into narrower channels, catching the soil which is washed from the hill-sides and increasing the land area.

One of the most interesting soil-makers, wresting new territory from the ocean itself, is the mangrove along the coast of Florida. Not only does it reach out with its aerial roots, entangling in their meshes whatever litter may float about, and thus gradually building up the shore, but it pitches even its young brood into the advance of the battle, to wrestle with the waves, and gain a foothold as best it may.

Not less interesting in this respect is that denizen of the southern swamp, the bald cypress, with its curious root excrescences known as cypress knees, which, whatever their physiologic significance, are most helpful in expediting changes of water into land sufficiently dry to be capable of supporting the more fastidious species in regard to moisture and conditions.

On the dry hot mesas, and in the arroyos of the south-western

tier of our States and Territories, we meet a different set of skirmishers following up the huge cacti and agaves, which together with the tree yuccas, penetrate into the very desert. In these regions the mesquite or algaroba and others of the acacia tribe form the second phalanx, as it were, gradually advancing their lines in spite of adverse conditions. In other regions the pine, satisfied with but scanty favour of soil moisture, and the spruce, able to sustain life in shallow soil, and the fir, in the higher, colder, and wetter elevations, sometimes much stunted, form the skirmish line. These improve the soil in its moisture conditions by their shade, and by the foliage and litter falling and decaying they deepen the soil, forming a humus cover. The duff that is found covering the rocky subsoil of the Adirondacks is formed in this way at the rate of about one foot in 500 years. They are soon followed by the birch, maple, elm, and ash, and in moister situations by the oak—first, that hardy pioneer, the black oak tribe, and then the more fastidious white oak, with whom the slower but persistent hickories, beeches, and other shade-enduring species begin to quarrel for the right of occupancy of the ground, until the battle is no longer that of the forest against the elements and lower vegetation, but between the mighty conquerors themselves. This struggle we can see going on in our primeval forests, wind, storms, and decay acting as allies now to one, now to the other side, and thus changing the balance of power again and again.

In this struggle for supremacy between the different arborescent species the competition is less for the soil than for the light, especially for tree growth. It is under the influence of light that foliage develops, and that leaves exercise their functions and feed the tree by assimilating the carbon of the air and transpiring the water from the soil. The more foliage and the more light a tree has at its disposal, the more vigorously it will grow and spread itself.

Now the spreading oak or beech of the open field finds close neighbours in the forest, and is narrowed in from all sides and forced to lengthen its shaft, to elevate its crown, to reach up for light, if it would escape being overshadowed, repressed, and perhaps finally killed by more powerful densely-foliaged competitors.

The various species are differently endowed as regards the amount of light which they need for their existence. Go into the dense forest and see what kinds of trees are vegetating in the dense shade of the older trees, and then go into the opening recently made, an abandoned field or other place, where the full benefit of light is to be had by all alike, and one will find a different set altogether occupying the ground and dominating. In the first case there may be found, perhaps, beech and sugar maple or fir and spruce; in the second case aspen, poplar, willow, soft maple, oak or pine, tamarack, &c.

All trees thrive ultimately best in full enjoyment of light. But some, like those first mentioned, can at least subsist and their foliage functionate with a small amount—they are shade-enduring kinds, usually having a dense foliage, many leaves, and each one needs to do but little work—and exert considerable shade when fully developed. Those last named, however, are light-needing kinds, and having less foliage, cannot exist long without a considerable amount of light.

To offset this drawback in the constitution of these latter, nature has endowed them as a rule with the capacity of rapid height growth, to escape their would-be suppressors; but again, what they have gained in the rapidity of development they lose in the length of life. They are mostly short-lived species, while the shade-enduring are generally slower growers, but persistent and long-lived. Some kinds, like most of the oaks, stand between the two; while exhibiting a remarkable capacity of vegetation in the shade, they are really light-needing species, but comparatively slow-growers and long-lived. One of the same species behaves also somewhat differently under different soil and climatic conditions; for instance, as a rule, the light-needing species can endure more shade on moist soils, and the shade-enduring require more light on drier soils.

In the earliest stages of life the little seedlings of most trees require partial shade, and are quite sensitive in regard to light and conditions. Some have such a small range of light and shade endurance that, while there may be millions of little seedlings sprouted, they will all perish if some of the mother trees are not removed and more light given; and they will perish equally if the old growth is removed too suddenly, and the delicate leaf structure, under the influence of direct sunlight, is made to exercise its functions beyond its capacity.

Left to itself the forest grows up, and as the individual trees develop, each trying to hold its ground and struggling for light, a natural thinning takes place, some trees lagging behind in growth and being shaded out, until in old age only as many trees remain as can occupy the ground without incommoding each other.

This struggle among the individuals goes on during their entire life. Some few shoot ahead, perhaps, because of a stronger constitution or some favourable external cause, and over-tower their neighbours. These, lagging behind, fall more and more under the shading influence of their stronger neighbours until entirely suppressed, when they only vegetate until they die. The struggle continues, however, among the dominant class, and it never ends.

Thus the alterations of forest growth take place, oak following pine, or pine following oak; the poplar, birch and cherry appearing on the sunny burns, or the hickory, beech, and maple creeping into the shadier pine growths. While in the eastern forests under natural conditions the rotation of power is accomplished in at least from 300 to 500 years, the old monarchs of the Pacific, towering above all competitors, have held sway 2000 or more years. In this warfare, with changes in climatic and soil conditions going on at the same time, it may well occur that a whole race may even be exterminated.

The study of the formative period of the forest is necessary in order to show clearly that the virgin forest is a product of long struggles, extending over centuries, nay, thousands of years. Some of the mightiest representatives of the old families, which at one time of prehistoric date were powerful, still survive, but are gradually succumbing to their fate in our era.

The largest of our eastern forest trees, reaching a height of 140 feet and diameters up to twelve feet, the most beautiful and one of the most useful, the tulip tree (*Liriodendron tulipifera*), is a survivor of an early era, once widely distributed, but now confined to eastern North America, and doomed to vanish soon from our woods through man's improper partisanship.

Others, like the Torreya and Cupressus, seem to have succumbed to a natural decadence, if we may judge from their confined limits of distribution. So, too, the colossal sequoias, remnants of an age when things generally were of larger size than now, appear to be near the end of their reign, while the mighty taxodium or bald cypress, the big tree of the East, still seems vigorous and prosperous, being able to live with wet feet without harm to its constitution, weird with the grey tillandsia or Spanish moss.

Having thus scanned through the traditions of unwritten history of the battle of the forest, having seen some of the combatants in the struggle, and learned something of their methods of conquering the earth and each other, we may take a look at the condition of things on the North American continent as it presumably was in the beginning of historic times or within our century.

As far as occupancy of the soil by the forest is concerned, we find that the struggle had not yet been determined in its favour everywhere. While a vast territory on the Atlantic side and a narrower belt on the Pacific Coast, connected by a broad belt through the northern latitudes, was almost entirely under its undisputed sway, and while the backbone of the continent, the crest and slopes of the Rocky Mountains, was more or less in its possession, there still remained a vast empire in the interior unconquered.

Of parts of this territory we feel reasonably certain from strong evidences that the forest once occupied them, but has been driven off by aboriginal man, the firebrand taking sides with the grasses, and the buffalo probably being a potent element in preventing re-establishment. In other parts it is questionable whether the lines along the river-courses, the straggling trees on the plateaus and slopes, are remnants of a vanquished army or outposts of an advancing one. In some parts, like the dry mesas, plateaus and arroyos of the interior basin and the desert-like valleys toward the southern frontiers, it may reasonably be doubted whether arborescent flora has more than begun its slow advance from the outskirts of the established territory.

Certain it is that climatic conditions in these forestless regions are most unfavourable to tree growth, and it may well be questioned whether in some parts the odds are not entirely against the progress of the forest.

Temperature and moisture conditions of air and soil determine ultimately the character of vegetation, and these are

dependent not only on latitude, but largely on configuration of the land, and especially on the direction of moisture-bearing winds with reference to the trend of mountains.

The winds from the Pacific Ocean striking against the coast range are forced by the expansion and consequent cooling to give up much of their moisture on the windward side; a second impact and further condensation of the moisture takes place on the Cascade range and Sierra Nevada. On descending, with consequent compression, the wind becomes warmer and drier, so that the interior basin, without additional sources of moisture and no additional cause for condensation, is left without much rainfall and with a very low relative humidity, namely, below 50 per cent. The Rocky Mountains finally squeeze out whatever moisture remains in the air currents, which arrive proportionally drier on the eastern slope. This dry condition extends over the plains until the moist currents from the Gulf of Mexico modify it. Somewhat corresponding, yet not quite, to this distribution of moisture, the western slopes are found to be better wooded than the eastern, and the greater difficulty of establishing a forest cover here must be admitted; yet since the forest has the capacity of creating its own conditions of existence by increasing the most important factor of its life, the relative humidity, the extension of the same may only be a question of time.

Temperature extremes, to be sure, also set a limit to tree growth, and hence the so-called timber line of high mountains, which changes in altitude according to the latitude.

If now we turn our attention from the phyto-topographic consideration of the forest cover to the phyto-geographic and botanical features, we may claim that the North American forest, with 425 or more arborescent species, belonging to 158 genera, many of which are truly endemic, surpasses in variety of useful species and magnificent development any other forest of the temperate zone, Japan hardly excepted. In addition there are probably nowhere to be seen such extensive fields of distribution of single species.

These two facts are probably explained by the north-and-south direction of the mountain ranges, which permitted a re-establishment after the Ice Age of many species farther northward, while in Europe and the main part of Asia the east-west direction of the mountains offered an effectual barrier to such re-establishment, and reduced the number of species and their field of distribution; nor are the climatic differences of different latitudes in North America as great as in Europe, which again predicates greater extents in the fields of distribution north and south. On the other hand, the differences east and west in floral composition of the American forest are greater than if an ocean had separated the two parts instead of the prairie and plains. This fact would militate against our theory that the intermediate forestless region was or would be eventually forested with species from both the established forest regions, if we did not find some species represented in both regions and a junction of the two floras in the very region of the forestless areas.

(*To be continued.*)

SOCIETIES AND ACADEMIES.

LONDON.

Zoological Society, November 20.—Sir W. H. Flower, K.C.B., F.R.S., President, in the chair.—Mr. F. G. Parsons read a paper on the anatomy of *Atherura africana*, compared with that of other porcupines. In addition to the points mentioned by Drs. Gray and Günther, as differences between the skulls of *A. africana* and *A. macrura*, the arrangement of the fronto-nasal suture, the position of the maxillo-malar suture, and the frequent presence of an "*os anti-epilepticum*" were noticed.—A communication from Mr. J. T. Cunningham treated of the significance of diagnostic characters in the Pleuronectidæ.—Mr. A. Smith Woodward read a description of the so-called Salmonoid fishes of the English Chalk, dealing with the osteology of *Osmeroides lewesiensis*, *Elopopsis crassus*, and *Aulolepis typus*.—Mr. W. Garstang read a paper on the Gastropod *Colpodaspis pusilla* of Michael Sars. He described a specimen of this rare mollusk found by him at Plymouth in the early part of the year.—A communication from Mr. A. D. Bartlett gave an account of the recent occurrence in the Society's menagerie of a case of one boa swallowing another of nearly equal size.—A communication from Prof. R. Collett contained a description of a new Agonoid fish from Kamtschatka, proposed to be called *Agonus gilberti*.

Royal Meteorological Society, November 21.—Mr. R. Inwards, President, in the chair.—Dr. H. B. Guppy read a paper on suggestions as to the methods of determining the influence of springs on the temperature of a river as illustrated by the Thames and its tributaries. The methods suggested were (1) comparison of the curves of the monthly means of the temperatures of the air and of the water for the river under observation with those of a river beyond the controlling influence of springs; (2) comparison of the monthly means of the temperature of the river under investigation with that of a river beyond the control of the springs; (3) comparison of the range of the monthly means of the river temperature with that of the air in the shade; (4) comparison of the daily range of water temperature at different stations along a river's course; (5) comparison of sunrise observations made at different stations along a river's course; (6) comparison of observations made at different stations along a river's course at the hour of maximum temperature; (7) comparison of the results obtained from a single series of observations made in one day along the whole course of a small tributary like the Wandle, or along the upper course of a larger tributary as the Kennet; and (8) determination of the distance from its sources at which the river begins to freeze.—Mr. Eric S. Bruce exhibited and described some lantern slides showing the disastrous effects of the great gale of November 17 and 18, 1893, upon trees in Perthshire, Scotland.—Mr. Alfred B. Wollaston gave an account of the formation of some water-spouts which he had observed in the Bay of Bengal.

CAMBRIDGE.

Philosophical Society, November 12.—Prof. J. J. Thomson, President, in the chair.—On the inadequacy of the cell theory and on the development of nerves, by Mr. A. Sedgwick. The author pointed out that the cell-theory, in so far as it implied that the organism was composed of cell-units derived by division from a single primitive cell-unit, the ovicell, would not bear the scrutiny of modern embryology, and that in fixing men's attention too much upon the cell as a unit of structure, it had had a retarding influence on the progress of the knowledge of structure. He illustrated this latter point by reference to the current ideas on two important subjects: the structure of the embryonic tissue called mesenchyme, and the development of nerves. The mesenchyme is not composed of separate branched cells, but has rather a spongy or reticulate structure, and is continuous both with ectoderm and endoderm. Nerves do not develop as outgrowths of the central organ, but arise *in situ* from the mesenchyme.—Note on the evolution of gas by water-plants, by Mr. F. Darwin.

PARIS.

Academy of Sciences, November 19.—M. Lœwy in the chair.—After the reading of the *procès verbal*, the meeting was adjourned as a mark of respect to the late Czar of Russia.

AMSTERDAM.

Royal Academy of Sciences, October 27.—Prof. Van de Sande Bakhuyzen in the chair.—Mr. Franchimont, in presenting Mr. H. van Erp's thesis for the Doctorate in Chemistry at the University of Leyden, entitled "Studie ober aliphatische nitraminen," described it as a summary of all the known acid and neutral nitramines and nitramides, and also of their modes of formation. In dealing with the action of water, acids and alkalies on these bodies, Mr. van Erp considers them as derived from the amide of nitric acid, and compares them to the analogous derivatives of nitrous acid, hypochlorous acid, &c. For experimental purposes he made the unknown butyl- and hexyl-derivatives: nine urethanes, seven nitro-urethanes, four acid nitramines with several salts, two mixed neutral nitramines. He failed, however, to obtain nitro-compounds of the tertiary butyl amidoformates. He has observed that while the potassium salts of the acid nitramines yield the neutral methyl-derivatives by the action of methyliodide, the silver salts produce an isomeric methylated nitramine, or a mixture of the two. Similar observations in the case of the salts of phenylnitramine were made later by Bamberger. The behaviour of acid nitramines towards dilute sulphuric acid was studied on hexylnitramine, the result being N_2O, two hexanoles, a primary and a secondary

(2 ?), hexene (1) and a dihexylic ether. Mr. van Erp has also observed the behaviour of neutral nitramines with alkalies. Dimethylnitramine gives nitrous acid, monomethylamine, formic acid (and methyl alcohol ?). Diethyl- and dipropylnitramine seemed not to be changed. Normal butylmethylnitramine, less easily than dimethylnitramine, gives nitrous acid, butylamine, formic acid (and methyl alcohol ?). It therefore seems that mixed nitramines give the amine with the greatest alkyl, or the methyl radical is most easily separated from the nitrogen.—On quadrinodal quintics, by Mr. Jan de Vries.—On the cranial nerves of vertebrates in amphioxus, by Mr. van Wijhe. The olfactory nerve represents a type of its own. The ventral nerves, or nerves of the myotomes, do not exhibit special characteristics. Among the dorsal or septal nerves, the trigeminal, facial, glossopharyngeal and vagus nerves could be recognised with more or less probability, chiefly by their relations to the branchial clefts, the first of which on the left side becomes the opening of the velum.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, NOVEMBER 29.

SANITARY INSTITUTE, at 8.—Workers in Copper, Zinc, Brass, and Tin: Dr. R. M. Simon.

FRIDAY, NOVEMBER 30.

ROYAL SOCIETY, at 4.—Anniversary Meeting.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Sub-aqueous Excavation at Newry: C. H. Olley.

SANITARY INSTITUTE, at 8.—Sanitary Law: Prof. A. Wynter Blyth.

SUNDAY, DECEMBER 2.

SUNDAY LECTURE SOCIETY, at 4.—Village Life in India: Mr. R.W. Frazer.

MONDAY, DECEMBER 3.

SOCIETY OF ARTS, at 8.—Modern Developments in Explosives: Prof. Vivian B. Lewes.

SOCIETY OF CHEMICAL INDUSTRY (Burlington House), at 8.—The Rational Sterilisation of Alimentary Liquids: Mr. E. W. Kuhn (of Paris).—An Investigation of the Natural Sodium Sulphate Lakes of Wyoming, U.S.A.: Dr. D. H. Attfield.—Specimens of India-rubber, and Petroleum Oil, Varnish, and Soap will be exhibited by Mr. Thos. Christy.

VICTORIA INSTITUTE, at 4.30.—Semitic Languages: Mr. T. G. Pinches.

TUESDAY, DECEMBER 4.

ZOOLOGICAL SOCIETY, at 8.30.—On some Points in the Anatomy of Ornithorhynchus paradoxus: Mr. T. Manners Smith.—On certain Points in the Visceral Anatomy of Ornithorhynchus: Mr. F. E. Beddard, F.R.S.—On some Remarkable Corals of Great Size from North-West Australia: Prof. F. Jeffrey Bell.—Second Report on Additions to the Lizard Collection in the Natural History Museum: Mr. G. A. Boulenger, F.R.S.

INSTITUTION OF CIVIL ENGINEERS, at 8.—The Machinery of War-Ships: Mr. Albert J. Durston.—Colliery Surface-Works: Mr. E. B. Wain.

ROYAL STATISTICAL SOCIETY, at 4.45.—The Eleventh United States Census: Hon. R. P. Porter.—Exhibition of the Hollerith Electrical Counting Machine: Dr. H. Hollerith.

WEDNESDAY, DECEMBER 5.

SOCIETY OF ARTS at 8.—Fire Protection: Edwin O. Sachs.

GEOLOGICAL SOCIETY, at 8.—Supplementary Note on the Narborough District (Leicestershire): Prof. T. G. Bonney, F.R.S.—The Tarns of Lakeland: Mr. J. E. Marr, F.R.S.—The Marble Beds of Natal: Mr. David Draper.—Description of a New Instrument for Surveying by the Aid of Photography, with some Observations upon the Applicability of the Instrument to Geological Purposes: Mr. J. Bridges Lee.

ENTOMOLOGICAL SOCIETY, at 8.—A List of the Lepidoptera of the Khasia Hills, Part III.: Colonel Charles Swinhoe.—A Monograph of British Braconidæ, Part VI.: Rev. T. A. Marshall.—On the Longicorn Coleoptera of the West India Islands: Mr. Charles J. Gahan.—Notes on the Fungus Growing and Eating Habit of Sericomyrmex opacus, Mayr.: Mr. F. W. Urich.—An Apparent Case of Sexual Preference in a Male Insect: Prof. E. B. Poulton, F.R.S.

THURSDAY, DECEMBER 6.

OYAL SOCIETY, at 4.30.—Experimental Researches on Vegetable Assimilation and Respiration. No 1. On a New Method for Investigating the Carbonic Acid Exchanges of Plants. No. 2: On the Paths of Gaseous Exchange between Aerial Leaves and the Atmosphere: Mr. F. F. Blackman.

SOCIETY OF ARTS, at 4.30.—Roman and British Indian Systems of Government: Hon. W. Lee-Warner, C.S.I.

LINNEAN SOCIETY, at 8.—A New Revision of Dipterocarpeæ, with Lantern Slides: Sir D. Brandis, F.R.S.—On the Spinning Glands in Phrynus: Mr. H. M. Bernard.

CHEMICAL SOCIETY, at 8.—The Use of the Globe in the Study of Crystallography: J. Y. Buchanan, F.R.S.—Latent Heat of Fusion: Mr. H. Crompton.—New Method of Preparing Dihydroxytartaric Acid: Mr. H. J. H. Fenton.—Essential Oil of Hops: Mr. A. C. Chapman.

LONDON INSTITUTION, at 6.—The Fauna of Rivers and Lakes: Prof. Sydney Hickson.

FRIDAY, DECEMBER 7.

ROYAL INSTITUTION, at 5.—General Monthly Meeting.

GEOLOGISTS' ASSOCIATION, at 8.—Note on Megalosaurian Teeth, discovered by Mr. J. Alstone in the Portlandian of Aylesbury: Mr. A. Smith Woodward.—On the Geology of the St. Gothard Pass: Mr. H. W. Monckton.

SATURDAY, DECEMBER 8.

ROYAL BOTANIC SOCIETY, at 3.45.

BOOKS, PAMPHLETS, and SERIALS RECEIVED

BOOKS.—Imperial University of Japan, Calendar 1893-4 (Tokyo, Maruya).—Catalogue of the Snakes in the British Museum (Natural History): G. A. Boulenger, Vol. 2 (London).—The Flower of the Ocean, the Island of Madeira: Surgeon-General C. A. Gordon (Baillière).—Topographische Anatomie des Pferdes: Ellenburger and Baum, Zweiter Teil (Berlin, Parey).—Butterflies and Moths (British): W. Furneaux (Longmans).

PAMPHLETS.—Sulle Oscillazioni Elettriche a Piccola Lunghezza d'Onda, &c.: Prof. A. Righi (Bologna).—North of England Institute of Mining and Mechanical Engineers: Report of the Proceedings of the Flameless Explosives Committee. Part 1. Air and Combustible Gases: A. C. Kayll (Reid).—Resultate der im Sommer 1893 in dem Nördlichsten Theile Norwegens ausgeführten Pendelbeobachtungen: O. E. Schiötz (Kristiania, Dybwad).

SERIALS.—Cassell's Magazine, December (Cassell).—Proceedings of the Aristotelian Society, Vol. 2, No. 3, Part 2 (Williams).—Transactions and Proceedings of the Botanical Society of Edinburgh, Vol. xx. Part 1 (Edinburgh).—Proceedings of the American Academy of Arts and Sciences, new series, Vol. xxi. (Boston).—Brain, Part lxviii. (Macmillan).—Kryptogamen-Flora von Schlesien, iii. Band, 2 Hälfte, 3 Liefg. (Breslau, Kern).—Journal of the Institute of Jamaica, September (Sotheran).—Longman's Magazine, December (Longmans).—Chambers's Journal, December (Chambers).—Natural Science, December (Macmillan).—Good Words, December and Christmas (Isbister).—Sunday Magazine, December and Christmas (Isbister).—Century Magazine, December (Unwin).—Humanitarian, December (Hutchinson).—Udgivet af den Norske Gradmaalings-Kommission, Vandstansobservationer, v. Hefte (Christiania, Fabritius).—Botanische Jahrbücher, &c., Zwanzigster Band, 1 and 2 Heft (Leipzig, Engelmann).

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